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SCIENCE AND THE SUPERNATURAL[1]

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I INTEND to make this discourse more modest than the title, and I trust you will find it so. Science is one, at least in its essential element, the method of reaching approximate truths. But scientists are many. On the topic before us it is preposterous for any man to speak for science as a whole and, by inference, for all scientists. We have scientists who still pray to the gods, scientists who laugh at the gods, and some who neither pray nor laugh, because they think they understand. I am sure all you expect of me this evening, and certainly all I intend to do, is to discuss the supernatural in the light that years of service in the science of physiology have given me. The topic of this discussion is not of my own selection. The views are my own. But they are neither unique nor original, except in the sense of being derived from cogitation on the common life, cogitations disciplined by years of research. I am not foolish enough to pretend that I am about to present to you anything that is both new and true. There are able tomes on the nature of science; and literature, *ad infinitum*, on the supernatural, especially in religions. There are able works on the conflicts between science and the supernatural. There are attempts at reconciliation of the supernatural with science. We have, in print, confessions of faith in traditional religions by otherwise competent scientists. We have, also in print, rejections of the supernatural by preachers and teachers of religion. I assume you are familiar with some, if not all, of this literature. Everything I am going to say has already been said, perhaps better, by other people. Nevertheless, here is the confession of a physiologist of lack of faith in the supernatural, and his reasons.

[1] William Vaughan Moody Lecture, University of Chicago. This address was first published in *Science*, 73: 217–225, 1931. It was suggested by a reader of THE SCIENTIFIC MONTHLY that it be reprinted here to remind some and to inform others of the outlook on science and life of a distinguished physiologist, now president of the American Association for the Advancement of Science.

SCIENCE

It is scarcely necessary, before this audience, to go into detail as to what we understand by science, although the term is frequently used loosely and with very different connotations. Probably the most common meaning of science is a body of established, verifiable and organized data secured by controlled observation, experience or experiment. Such data frequently lead to an approximate understanding of the causal relations between events, and these relations give us the so-called laws of science. To my way of thinking, the element in science of even greater importance than the verifying of facts, the approximation laws, the prediction of processes is the method by means of which these data and laws are obtained and the attitude of the people whose labor has secured them. In other words, the most important element in science appears to be the scientific method. What is the method of science? In essence it is this—the rejection *in toto* of all non-observational and non-experimental authority in the field of experience. No matter how high in state, church, society or science the individual may be who makes pronouncement on any subject, the scientist always asks for the evidence. When no evidence is produced other than personal dicta, past or present, "revelations" in dreams, or the "voice of God," the scientist

can pay no attention whatsoever, except to ask: How do they get that way? If evidence is produced, he proceeds to examine the evidence. Does the evidence justify the conclusions or statements made? There is nothing recondite or abstruse in the method of science. To be sure, in many fields of scientific research methods of approach, methods of experimentation and data leading to certain or probable conclusions are becoming increasingly so recondite and specific that laymen in general and, in fact, scientists in other fields, are unable to follow, but the principle of the method is simple enough, and that this method of approach will give us the closest approximation to understanding and truth that we are able to reach to-day I think will be agreed to by all informed people.

The principle of the scientific method, in fact, is only a refinement, by analysis and controls, of the universal process of learning by experience. This is usually called common sense. The scientific addition to common sense is merely a more penetrating analysis of the complex factors involved, even in seemingly simple events, and the necessity of numerous repetitions and controls before conclusions are established. Where laymen, as a rule, do not understand or apply the scientific method is in the matter of controls. Thousands of honest errors have been committed and ludicrous conclusions promulgated by failure to understand the necessity of controls. Illustrative instances of this may be cited from the field where I have most experience, namely, physiology and disease. Fortunately, man recovers, as a rule, spontaneously from many diseases, such as colds, pneumonia, typhoid fever, headaches, diarrhea, etc. To be sure, some of these diseases may also lead to death, but if the person having these ailments does not die in the process of the malady, there is more or less complete recovery. Now, if the person not aware of this has the notion handed to him by his father, his priest or his mythology that holy water, holy oil, an amulet, a prayer, the killing of a goat or the laying on of hands will cure these diseases, experience will teach him that after applying any one or all of these measures to the sick persons many of them do get well. Indeed, applying all these to the sick might be a kind of control because a thinking person might be led to wonder which of these measures was the most potent in reestablishing health, and such questioning might lead him to try whether the person might recover without any of them. But usually this is not done. Those who believe that ill health can be cured by prayer will pray. Those who believe that an amulet is a cure will apply the amulet, and those who have faith in holy oil or laying on of hands will try these methods, and most of the people get well. A true statement of the facts is that sick persons so treated do get well after the treatment. The common error made is that the person recovers because of the treatment. The experience is correct. The conclusion is wrong. There is no control. The obvious control, of course, is a sufficient number of people of the same age with the same malady and none of the above measures applied, and the duration of their illness and percentage of recovery contrasted with the treated group. Until consciousness of the necessity of controls in all endeavors to ascertain new truths or in evaluating current theories, dogmas or practices, until this consciousness has become a compelling factor in society, man remains essentially unscientific no matter how much detailed scientific facts he may remember and how much scientific patter he may have absorbed. He is like the rooster who crows every morning before daybreak, notices that a little later the sun rises, and then concludes that it is his crowing which brings the sun above the horizon.

It seems that the supernatural in the sense of religions or a religious attitude toward nature and life is nearly universal among men at some stage of development. Science in the sense of elements of the scientific method, the learning by experience, is even more universal. It antedates man. The amoeba appears to work in part by the principle of trial and error; so do some of the higher animals, including the ape. This type of reaction or behavior in the simpler forms of animal life does not necessarily connote conscious associative memories, but there is no good reason for denying the latter factor in the higher animals. The trial and error method is direct experience. Experience is experimentation in embryo. The

very fact that every known race or tribe of humans has changed (some say progressed or improved) in the practical arts of living, in mores, in social organization and in religion is evidence of some learning by experience, despite all the retarding force of tradition and myths, and despite the absence of conscious control and analysis. Learning by experience, however, can not be dignified as science until a critical analysis of the factors and rigid controls of experience are introduced.

The *attitude* of the scientist is also an important factor in application of scientific method and therefore in the science itself, or at least in the growth of science.

This attitude is, of course, partly characterized by challenge of authority, be it man or God. It is further characterized by a serious attempt on the part of the scientist to control his own emotions and his own wishes in the matter. The scientist is, after all, an ordinary human being and the control of his desires, emotions or wishes in a problem is seldom, if ever, one hundred per cent. The scientist tries to rid himself of all faiths and beliefs. He either knows or he does not know. If he knows there is no room for faith or belief. If he does not know he has no right to faith or belief. He may have grounds for hypotheses, but the moment he begins to have faith in his hypotheses the hypotheses tend to become myths. One of my teachers in zoology used to say to us: "Friends, it is necessary, at present, to entertain theories in zoology, but we must be on our guard against being entertained by these theories." These elements of the scientific attitude I have indicated are scientific ideals which few, if any, scientists are able to attain all the time, particularly when they are dealing with matters of tradition or matters to which they are emotionally conditioned in early youth.

The term science is sometimes limited to the fields of mathematics, astronomy, chemistry, physics, geology, biology, and their practical applications. This may be due partly to the fact that in these fields we have to-day the greatest body of verifiable data and so-called laws of science. However, one of the elements in the scientific attitude is the application of the scientific method to the entire universe, including all human experience and all human relations. The man of science seeks for evidence in the case of all traditional beliefs and practices, and he must abstain from positive views when evidence is lacking in these fields just as he does when evidence is lacking in his own particular field of endeavor. Of course, it is much more difficult to apply the method of science to such fields as religion, social customs, political and economic institutions. Scientific controls are not readily devised or secured, but the application of the method of science in these fields has justified itself by results. It has afforded us a better understanding of the origin of our social heritage, even if it has not to date yielded any considerable body of verified data or laws similar to that of physics, chemistry, geology or biology. To the oft-repeated question—Are psychology, sociology, economics, etc., real sciences?—I would answer: They are, to the extent that the rigid application of the scientific method and scientific attitude is pursued by the people who cultivate these fields. The biologist is confronted in his own field by some of the difficulties that other scientists experience when they enter the fields of sociology, political science, psychology or religion. The past seems to be behind us despite the idea recently advanced that time may actually run backwards. Time may run backwards or in circles in the mind of the philosopher, but it does not seem to run backwards in biology. At any rate the history of the early ancestors of living plants and animals, and possibly the very origin of life itself, is only dimly written in the strata of the earth of bygone geological epochs. We may discover and describe a link here and there, but many of the links are as yet missing. We can not experiment with the past, we can not establish controls. Not only that, but many of the processes in the life of the individual man, animal or plant of to-day appear to be as complex, as difficult to analyze and separate and therefore control as the complex forces in society.

So much for science. If we have rightly understood and correctly outlined the method, attitude and scope of science, we might stop right here, and let you draw your own conclusions as to the supernatural, the

"holy," the "divine." It might be better thus, for those of you who have already done so will be bored by the rest of this discussion; and others might take the conclusions on my mere say so, or reject them because contrary to their faith. In either case further discussion is largely futile.

The Supernatural as a Way to Knowledge

By supernatural we understand information, theories, beliefs and practices claiming origins other than verifiable experience and thinking, or events contrary to known processes in nature, such as the production of wine from water alone; the resurrection from the dead of persons in advanced stages of decomposition; accounts of creation of the world and of man by people who were not present at these events, and not in a position to infer from cosmic data; specific codes of behavior enunciated directly to some man by some anthropomorphic god; arrest of the course of the sun through space so that the Jewish army could see to kill a few more natives; casting devils out of men, and sending demons into hogs; human pregnancies solely through non-material, that is, divine agencies; perpetual recurrence of a species of "immaculate conception" in that a divinity sends embryonic "souls" into every human fetus either at the moment of union of sperm and ova, or later in intrauterine life, etc., *ad infinitum, ad absurdum, ad nauseam.* This supernatural has been presented to man with varying degrees of clarity in a great variety of books and sermons by prophets, priests, and other holy men, in addition to the information in so-called sacred books, such as the Bible, the Koran, the Vedas and the book of Mormons. We all know that there are great variations among modern adherents of the sundry religions both in the amount that they individually accept and in interpretations put on what they do accept of this supernatural. But the supernatural in this sense is found at present in the theories, beliefs and practices of most, if not all, religious groups. We find a sprinkling of it here and there in social habits, customs and ethics. At one time it was prominent in political institutions and theory, but in most parts of the world "rule by divine rights" has been abolished, at least in theory. The gradual elimination of the supernatural and the divine in governments can not be primarily credited to science or scientists. It was forced by the travails of the common life. The supernatural sanction in social customs, habits or ethics frequently touches matters of relatively little importance, such as the interdiction of eating pork for the Jew, and the eating of meats on Friday for the Catholic, the ritual of baptism in the Christian religion, the circumcision in the Jewish religion, shaving or not shaving the face or the head, etc. The dogma that each individual marriage, when solemnized by a priest, is a "sacrament" made in Heaven by Almighty God and holds "till death do them part" has a more practical significance.

I am no authority in this field, but according to the best information now available it would appear that early mores had little or no connection with the supernatural. The grafting of the supernatural on ethics appears to come relatively late in human history. At any rate, so far as the essential mores of practical living is concerned, similar principles have been developed in various social groups independent of, parallel with, or under the influences of a variety of religious beliefs. These, therefore, have the sanctions of social necessity, convenience, or safety quite apart from the supernatural.

The supernatural is particularly abundant in the field of religions. I shall not attempt the impossible, *viz.*, a definition of religion. We have, however, people who entertain religious theories and follow religious rituals. When I speak of the Jewish, the Christian or the Mohammedan religions I refer to the theories, practices and attitudes peculiar to the people in these religious groups. I think we may get somewhere in the discussion if we treat religion in this way rather than by abstract definition. Most religions have in common the view and belief that some time somewhere God or gods, supernatural beings communicated to man information on the origin and nature of the universe, on the origin and nature of man, on the nature and control of the forces of nature about us, on the nature of evil, etc. In most cases these so-called revelations occurred so long ago

that the person or persons receiving them are buried in obscurity and myths. We can not analyze the alleged facts and circumstances. Fortunately, we have in this country two recent divine revelations of this type, namely, Mormonism on the basis of the Book of Mormons, and Christian Science on the basis of the divine teaching of Mary Baker Eddy. These are so recent that the personalities and the circumstances involved are not yet entirely obscured by myths and lore.

What has science to say to all this? The most serious aspect of the supernatural is, not the revelations, *per se*, the miracles, the myths and the guesses, but the injunction that all this must be taken on faith, that inquiry and doubt is tabu—that is, sin. A good deal of "revealed" information about the nature of the world and the nature of man has proved entirely erroneous. So far as the nature of the world and of man is concerned the revelations appear to be nothing but what could have been projected as guesses by any human contemporary of the revelations, on the basis of the knowledge and the ignorance of those times. The "revelations" have been of no aid in the advance of real knowledge of cosmogony, physiology, physics, chemistry or disease. On the contrary, they have, through human stupidity and obscenity, frequently aided in retardation. The revelations to Joseph Smith (the Book of the Mormons), the repeated revelations by Jehovah to Brigham Young, and the rise of Christian Science are recent. The character, education, intelligence and environment of the people concerned are fairly well known. In the light of all the known facts in these instances, is there any intelligent man or woman to-day, not steeped in childhood in the lore of Mormonism or Christian Science, who can have any respect for such revelations as a source of knowledge? When the Mormon leaders received a tip from God that polygamy was ordered by him for his chosen people on earth (by the way, a revelation that is easy to take by the average human male), the United States Government did not hesitate to challenge God, or Brigham Young's sanity and veracity. The Federal Government was powerful and adamant and God yielded through a second revelation to the effect that he had changed his mind and polygamy was no longer according to the plan of God! In some cases the "revelations" are reported as coming through dreams; in other cases through brush fires; by direct writing of the finger of God on stones, or indirectly through oracles, popes, the flight of birds and the liver of slaughtered bulls.

The physiologist can not accept revelations from dreams any more than he can detect wisdom in hallucinations. The brush fire may reveal something of the nature of the world, but it can tell us nothing of the origin of man or the ways of the good life. The supernatural as a way to knowledge is in direct conflict with science.

That many intelligent people of to-day both inside and outside the religious groups reject much of the anthropomorphism of the gods and the more palpably absurd phases of the supernatural as a way to understanding is no news to you. They usually retain a distillate of the supernatural in form of beliefs in a "moral purpose" in the universe. And having injected human ethics into an obviously a-moral universe,[2] they endow man with personal immortality. This refined supernaturalism is still essentially anthropomorphic and homocentric. Even this form of the supernatural has no sanction in science or analyzed human needs, as I understand them.

The Content of the Supernatural Revelations

So far we have considered the supernatural mainly from the aspect of the way of learning, the way of extending knowledge, the way of greater approximation to truth. A word or two on the content of the supernatural seems in place here. Most of the weird stories of creation of the universe, animals, man, of divine or demoniacal control of natural forces, of disease, etc., that have come to us *via* the supernatural route

[2] The concept of moral and immoral behavior has developed in connection with normal human adults or any hypothetical personality, to whom we ascribe consciousness of "right" and "wrong," and a feeling of freedom of choice in behavior. "*A-moral*" signifies the absence of these elements of personality, as in the behavior of young children, animals, the insane, and the physico-chemical forces of the universe.

run contrary to facts now known, or rendered untenable, as possibilities, by known facts. Between the stories in the book of Genesis, as an article of faith, and the planetesimal hypothesis of Chamberlin and Moulton (probably the greatest intellectual achievement so far in the University of Chicago) as a working theory, science *must* choose the latter. Divine benevolence and wrath, devils and demons are not factors in health and disease, according to the data of modern medicine. Science and miracles are incompatible. Much happens in nature and in man for which science has yet no complete analysis of the causal chain. We recognize the unknown but not the unknowable. When we know that we don't know, that is itself an achievement, for then the field is cleared of the confusing and obstructing rubbish of tradition, and we are free to use all our ingenuity and imagination in contriving methods to find out. Miracles of sufficiently recent occurrence so that fair information is available of the alleged facts and circumstances are resolved into misrepresentation or misinterpretation of the facts. In regard to the recurring miracle of changing bread and wine into human flesh and blood by Christian rituals, biological and biochemical tests of the bread and wine after being subjected to such rituals reveal nothing but the original bread and wine. To persons fairly familiar with biology and physiology the theory of animal and human evolution and genetic relations is a closer approximation to what happened in the past than any guess or story in "sacred books" or mythology. I have a fair acquaintance with most of them.

The Theory of dual nature of man (body and "soul") and its equal: The theory of *personal immortality* appears to be partly of mythological and supernatural origin, partly philosophical. The alleged objective evidence of these views is entirely mythological and supernatural, unless we are to dignify as evidence the ancient and modern communications with the dead by clairvoyance, "psychic mediums" and "ectoplasm." When examined, the "ectoplasm" appears to go the way of all errors and frauds. I know these attempts, and I am still skeptical.

Has science anything to say on the theory of personal immortality? The idea of persistence of the individual after physical death came down to us from the ancients in most if not all races. What credibility are we going to give to the idea solely because of its venerable age? So far as I can see, we can give no greater credibility to the ancients' views on immortality than to their views on other things about which they knew nothing. Conscious phenomena and intelligence in man, that is, personality, appear to be just as much an evolution of the material world as is the rest of the body processes. We seem to be forced to this conclusion from the evidence of the intimate dependence of all phases of consciousness, memory, and personality on the quantity and quality of the nervous system, and these, in turn, depend on all the rest of the body mechanisms.

It is perfectly true that we can cut off an arm or leg, remove certain peripheral ganglia and even a certain limited part of the central brain without seriously interfering with consciousness or personality. We can leave the brain structure anatomically intact, and through poisons eliminate consciousness temporarily or alter the individual personality permanently. The data from brain tumors, brain injuries, drugs, such as sedatives, hypnotics and anesthetics, experimental physiology, defective heredity, show that there is a close correspondence or dependence of consciousness, intelligence, memory or individuality on the nervous system.

What is personality? I think biologists would agree, to-day, that one element in personality is heredity, the kind of germplasm with which we are endowed at conception.

In the case of man and other mammals the original germplasm is subjected to months of intrauterine environment. The latter is complex, not simple. Such material factors as the constitution, health and food of the mother appear to have a very real influence on the constitution of the fetus, and after birth such material factors as disease, accidental injuries, food, etc., may further materially modify the final product: man or woman.

The hereditary personality is further modified and built up gradually by experience and memory, so that to-day I am a somewhat different person from what I was twenty years ago. It seems at least highly

probable, on the basis of biology, physiology and medicine, that this experience or the cumulative effect of the environment depends on changes built up mainly in the nervous system. The modifications of the nervous system called memory are less stable than the hereditary elements of the nervous organization. All the present evidence points to the fact that at death the nervous system goes to pieces with the rest of the body. Indeed, the disintegration of the nervous system, and with it the personality, may start before the death of the individual. The tragedies of "second childhood," of the aphasias, of senile dementia are known to all informed people. It doesn't make any difference whether the disintegration is fast or slow. We may preserve for a time some externals by desiccation, embalming or petrification. But fossils and mummies are as dead as the ashes of the funeral pyre. I can not conceive of events and environments in the future that would exactly reproduce my heredity and personal experience. On the basis of the known and the probable, immortality of the person is, at present, untenable. Leaving, for a moment, the realms of knowledge and reason and speaking of personal wishes, of likes and dislikes, the wish for personal immortality may be an extension of the pleasure in living, sometimes called "the will to live." The quantity of these emotions appears to vary in different people. Many seem to find comfort in the theory of "Nirvana," the state of everlasting unconsciousness. "Nirvana" may, without trickery or undue violence to reason, be translated into what modern biology indicates as the end of the individual, but the ways of attaining "Nirvana" appear to me incompatible with the good life here. As for other conditions of existence of the individual after death, other abodes of the "souls," the sundry infernos arouse in me, not fear, but pity and wonder how man can choose to torture his mind with such cruel absurdities; and I have not seen any heaven described where I care to go. My forebears had their Valhalla with its mead, its roast pork, its combats; the American Indian his happy hunting grounds, the followers of Mohammed their heaven of houris; the Christian has his golden city of many apartments, his golden harp and his oriental worship of adulation. But hunting means destroying fellows not so very different from ourselves. A heaven of mead and pork and fights and females forever leaves me cold. Flowers, though they like ourselves last but for the moment, are finer than gold, and justice seems a better goal than worship. When the shadow lengthens I am content to call it a day and leave the work to others. The passing of personal immortality seems to have added interest to my work to-day, greater interest in my students, in my fellow men, in other things that seem worth-while human efforts. For when I die, I will be a long time dead.

I am perfectly well aware that many able and fine people inside and outside this hall will arise with impatience, if not in anger, and say: "Your analysis of the supernatural refers to an extinct species. It does not apply to religions or religious people of to-day. You are belaboring a man of straw." What are the facts? Is supernaturalism a thing of yesterday? Have the peoples of the earth ceased to chant every variant of the tune, "The old time religion is good enough for me"? If the orthodox Jew (and that embraces most of the Jews) has dethroned Jehovah, and rejected the Bible, I have not heard of it. According to the latest news the Pope is still God's viceroy among men and the faithful Roman Catholics still believe that the voice of the Vatican is the voice of God. The acceptance of the whole Bible as divine truth is not a rarity among Protestant Christians. The God of the Jews, the Christians and the Mohammedans in 1930 is not a fossil. Enter almost any religious service and you get an earful of ancient and modern supernaturalism anent the soul, the devil, hell and heaven, sin, redemption, almighty Gods, angels, divine purposes, prayer. Is the supernatural extinct? Take a look on and about this campus, and you will find a very prolific and very recent growth of chapels and churches: edifices, I am delighted to note, only in part dedicated to the rituals of the "God of old." To be sure the supernatural is not in our federal constitution. But it is not absent from state and municipal codes. "Acts of God" are embalmed in legal lore. Physiology and biology can not be taught at

public expense in the states of Tennessee, Arkansas and Mississippi because it conflicts with "revelation." Is supernaturalism dead? What I have said here to-night would subject me to arrest and imprisonment in the state of Massachusetts, and disqualify me as a witness in court in at least six other states in the Union. Yes, my friends, supernaturalism is dead, indeed! Let a Jew, a Roman Catholic, a Mohammedan, or a man of no belief, like myself, run for governor in any state south of the Mason and Dixon line (and possibly in some states nearer home), or for President of the United States and he will discover something! The world has, indeed, moved since the days of Galileo, but in some places it has not moved very far. Why, the handful of liberal and informed people who have worked their way out of a cocoon of supernaturalism does not even make a respectable leaven in the college graduate group!

President Hoover, an engineer, and therefore at home in science, stated in his recent Thanksgiving proclamation: "We have been blessed with distinctive evidence of Divine favor. As a nation, we have suffered far less than other peoples from present world difficulties." This statement appears to imply that a divinity controls economic contingencies and rainfall, and either that we as a nation are morally more worthy than other peoples harder hit by economic and natural events of the past year, or else that this divinity is unfair in decreeing punishments and favors.

According to several Italian churchmen, the recent disastrous earthquakes in Italy were caused, not by unbalanced stresses in the crust of the earth, but by the Christian God, as punishment for the sins of men, women and little children in the devastated areas! Granted, for the sake of the argument, that the bishops are right and modern geology is wrong about earthquakes, we may still ask for evidence that men, women and little children living in earthquake areas are greater sinners than people living elsewhere; and again, if questioning was not tabu, how can a *just* and *loving* god institute such indiscriminate punishment? How can a *just* and *almighty* God permit such catastrophes to occur?

Within the present year five Protestant pastors in our neighboring state, Kentucky, are reported to have published the following statement: "God will and can answer prayer for rain. God has never withheld rain from the earth except in a gracious effort to bring his own people back to the ways of righteousness and holiness." If there is a God both almighty and just, prayer for rain and all kinds of favorable weather anywhere is unassailable. But if rain is sent to earth in proportion to holiness and prayer to Jehovah, the sundry heathens and all worshipers of "false gods" would have a dry time, not to speak of plants and animals who, according to the Bible, have no souls to pray with. Unfortunately for that kind of faith, the rainfall in heathen Philippines (that is, before the introduction of Christianity) was greater than in our Christian state of Tennessee. But this question need not be left in the realm of faith and controversy. It can be settled by controlled observation. What is the ratio of rainfall to Christian, Jewish, Mohammedan or Mormon prayers in various lands? The states of Washington and Oregon (west of the Cascades) have more rain than has the state of California (west of the Sierras). Is this difference due to the wickedness of Hollywood, and the past generation of gold diggers, and the holiness of the lumberjack? The adherents of the supernatural pray and irrigate the arid lands; others merely irrigate. The crops seem to parallel the irrigation rather than the prayers. Were it not for supernatural tabus, many other supernatural claims could be put to the experimental test. It should not be much more difficult to determine the efficacy of prayer against such diseases as syphilis, malaria, diabetes and goiter, than to establish the merits of arsenic, quinine, insulin and iodine. Not very long ago I read a signed public statement by a lady in the state of Kansas to the effect that she had seen a goiter melt away from the neck of another lady during the praying of the Reverend Aimee Semple McPherson directed toward this end. This appears like direct evidence. But metabolism tests, neck measurements and motion pictures of the "melting" process would go further to convince the skeptics. What people under intense emotions and de-

sire to believe think they see has frequently no relation to the light that actually impinges on the retina. Intense faith as well as intense fear seem to predispose to hallucinations in many people.

The moral efficacy of infant and adult baptism could also be tested experimentally, although with less accuracy, until better quantitative measures of human character are worked out. A prominent physiologist told me he had done this experiment in his own family, having two of his children baptized, and keeping the other two children as controls. I will not even mention the results, for we draw no conclusion from so few experiments, but it might be pointed out that identical twins would be the best material for this test. Is supernaturalism dead? Some Protestant clergymen inform us that Jehovah is a prohibitionist, and the people who oppose our present prohibition of alcoholic beverages are fighting God. Some of the Roman Catholics tell us that raising grapes and drinking wine is God's plan for man. Such confusion on the celestial lines of communication ought to provoke thought. It seems to induce nothing but reciprocal anger.

Many people take the position that science is well and good in the "material" world. They would exclude the method and attitude of science from certain fields of human life. A prominent New York rabbi said only a few days ago: "Human feelings and emotions will remain outside the scope of science forever." As if the biological sciences, including medicine, have not already produced a very respectable body of verifiable data on the mechanisms of the emotions. The relation of the brain to the emotions is nearly as clear as the relations of the kidneys to the secretion of urine or the relation of the gullet to swallowing. That complete obstruction of the gullet will prevent swallowing I do not think would be denied even by a rabbi. It is a favorite saying that there is more than science in the universe and in human life. We grant that. At the present the unknown exceeds the known. There is more ignorance than science. But is that a cause for exultation? Instead of wasting time and energy in the futile effort of building fences around science, and in a meticulous labeling of the "unknowable," we had better join hands in tackling the unknown, not with worship, prayer or propitiation, but with the tool of science. Here is useful and joyful work for everybody.

The Ethics of the Supernatural

May I make a few concluding remarks on the ethics of the supernatural, speaking not as a scientist but as a common man? The ethics of science is simple: absolute honesty in recording and presenting data, and curbing wishes, personal prejudices and emotions by reason in interpreting the data.

There appears to be a great variety of ethics in the supernatural. Looking upon the supernatural simply as man's early stumbling attempts at learning, at adjustment, as flounderings toward greater happiness, as quests for explanations of the unknown, this variety is both inevitable and understandable. From this point of view, the modern man of science has no essential quarrels with Jesus, Confucius, Zoroaster, or Buddha. They did the best they could, considering the ignorance of their times. We can do no more. But now and then individual champions of the supernatural have been either unusually stupid or inordinately selfish and cruel. The judgment of posterity will be severe on the men who coerced Galileo and their brethren of to-day who know or might know, yet rivet the shackles of supernaturalism on the human mind. For they sin against man. It is significant that neither Jesus nor his apostles appear to have claimed any supernatural authority or absolute wisdom for their sayings or writings. The ignoble doctrine of divine revelation of absolute truth for all times appears to be a later invention. But in Mormonism and Mohammedanism it is present with the founders. I said: ignoble doctrine. Intellectual tyranny is to me as immoral as physical tyranny. Stifling freedom of inquiry and of thinking by religious tabus or legal dicta appears to me highly immoral.

The view or belief that some one man or group of men (such as Brahmins, popes, priests, etc.), above all other humans, is specially endowed or enlightened to perpetuate and advance truth, and mediate between gods and man favors tyranny. It

seems inimical to knowledge and human dignity, hence immoral.

The supernatural theories of "sin," personified evil, redemption, eternal damnation, etc., when actually believed, have created and are creating much disturbance in man's emotional life, in the way of fear, worry, melancholy, if not outright insanity. The theory or doctrine of the vicarious atonement in the Christian religion is not only a projection of views and practices of barbarism into modern life, but it connotes a principal of punishment and propitiation at variance with modern sense of justice. It goes without saying that many Christian people are not aware of this.

If we take a look at the gods, they can be understood and condoned as inventions of man, at varying stages of social development. The fossilization of nearly all so-called sacred books by edicts and tradition has brought about the anomalous condition that the best people in many religions to-day are ethically superior to their gods. In the recent invasion of Palestine the modern Israelites have shown themselves in treatment of the Arabs, by and large, superior to Jehovah of the Bible.

If man as well as his social environment remained stationary, static mores might serve very well. But social, economic and political life appears to be more fluid than man. Hence the necessity of continuous amendment of the mores. For example, the travail of modern life is forcing the practice of birth control into the open for a more rational and humane settlement, despite the thunders from Mount Sinai and the echoes thereof from the Vatican Hill. If a physiologist, in 1930, may venture to reinterpret the aphorism of Paul, anent faith, hope and charity, it would read something like this: Faith is of the past, hope must be chastened by experience, charity in modern garb, is misdirected benevolence. But there remains the endeavor towards understanding, the hunger for beauty, the urge for justice—these three, and the greatest of the three is justice.

Science nurtures inquiry, the supernatural stifles it. The two are in their very essence incompatible, but they can apparently coexist in some scientists of the first rank. Man is, indeed, a perplexing animal. He is rarely consistently consistent or consistently inconsistent. The crook is not always crooked, the murderer not always cruel, the thief not always greedy. An honorable person may lie and a liar sometimes tells the truth. A shrewd business man may consult a soothsayer and be afraid of a black cat. Most men in early childhood are emotionally conditioned to the supernatural, just as they become emotionally conditioned to other elements of childhood environment: parents, places, playmates, nursery rhymes, the old swimming hole, and what not. Retaining and recalling these emotions please us. Adults may be conditioned, but usually with less emotional content than the child. We can be conditioned to science or justice just as to the supernatural, but the latter usually gets there first. The conditioned emotions usually outlive one's intellectual metamorphosis. Their disappearance seems to be a slow atrophy of disuse. Many factors appear to enter into the persistence of early conditioning to the supernatural, such as group loyalty, the desire to conform to social usage, the disinclination to disturb or distress parents and other intimate friends; social, political and financial ambitions, etc. Men also appear to differ in the emotional satisfaction obtained from the mystic. Additional factors, such as individual emotional capacity, may be operative in making some scientists think and work, while others think and work and pray. I admit it may be easier for men in the physical sciences than for biologists to cling to the supernatural, for much of the grotesque in the supernatural concerns man and other living things rather than inanimate nature. But even so, it is a fact that Rev. Stephen Hale laid the foundation for the science of hemodynamics, and Friar Mendel discovered fundamental principles in heredity. So far as I know, the Reverend Hale and Friar Mendel were sincere adherents of their respective religious cults. Our social heritage, good, bad and indifferent, clings to us like the hand and the appendix of organic inheritance. Hence, like the proverbial Englishman, we "muddle" but, now and then, we "muddle through." Fear and faith have ruled much of man's past, but the millennium is still far, far away. Now let us

try what may be accomplished by undertaking. Give science a chance.

I seem to sense a silent sigh from you, saying: "Thank God, he is through." I am—nearly. Knowing next to nothing about public speaking I consulted an experienced colleague, before preparing this talk. He referred me to a well-known canon, which reads: First, you tell your audience what you intend to tell, then you proceed to tell it, and lastly you tell what you have just told. You may have observed that I have followed this advice. I have now reached the lastly. Lest I be accused of hiding my real views in a plethora of verbiage, I will attempt to sum up, in threescore words, what I tried to say in seven thousand: As I see it, the supernatural has no support in science, it is incompatible with science, it is frequently an active foe of science. It is unnecessary for the good life. And yet, the supernatural, in varying dilutions, is likely to persist in society for a very long time. The unconditioning and reconditioning of mankind in fundamentals has been a slow process in the past. It may go a little faster in the future. It is a matter of forgetting the hypothetical universe created out of ignorance and motivated by our undisciplined emotions; and a reconditioning to the actual universe, as gradually understood through controlled experience and experiment.

SCIENCE AND HUMANISM

The current discussion of values in college education seems determined to fix in the public mind a distinction between humanistic studies on the one hand and scientific studies on the other. It is a distinction that, in fact, does not exist. If the greatest humanists of past centuries were to be resurrected and introduced to modern knowledge, I think that they would be much more interested in science than in critiques on creative work already done in the humanities. They would be disappointed by the wide turn in college from creative work to talk about it; and they would doubt the purposes that are to be served in any civilization by such "exercises." They would deplore the glibness and cocksureness that generally mark men who graduate under glib and cocksure teachers absorbed in the study of dialectics to the neglect of judgment. They would be more inclined to enquire about the steps by which men reached the point of discovery and relationships of forces until recently unknown, and of textual interpretations unguessed by their precursors. They would be interested in the mental climates of successive decades that preceded given discoveries. They would want to know about the critical effects of a given enquiry upon further enquiry and the mental saltations and sequences that enabled a man to reach the goal of an important discovery.

I feel sure that they would also ask about the social consequences. They would probably be amazed at the extent to which these consequences are neglected by schoolmen, industrialists, and statesmen alike, until they are fastened upon the people. They might end by regretting, as we do, that social understanding and control have lagged behind the physical and biological sciences. But I cannot imagine them regretting that discoveries had been made. On the contrary, I think of them as wishing to jump in and help further discovery along and greatly excited by the opportunity.

Put in another way, these humanists of an earlier period would say that the thing of chief interest to them is discovery, and that new knowledge bearing on life and civilization is just as important in our already complex times as in past times. I cannot believe that we have made a sound educational plan, or improved our more or less established college curricula, until we have rubbed out the line that commentators keep drawing between the sciences and the humanities. It is the constant interplay between the new and the old, whatever the kind of knowledge it may be, between the accepted interpretation of past experience and the challenge to interpretation of present discovery, that gives us a balanced judgment and a wider and sounder application of knowledge to life.

It is for this reason in part that the history of science becomes of increasing importance. It is also for this reason that the training of men in the sciences in our graduate schools should not be merely professional or technical. It is neither safe nor true to draw generalizations about the deficiency of scientists or of humanists as groups: one can find bad examples in either group. What is important is that we should reduce the number of such bad examples. We should make a Ph.D. mean something more than proficiency in a small sector of a great subject. A man should be capable of thinking about the meanings and the applications of his science, and not merely about a job in teaching "courses" or advancement in salary in an industry by knowing enough to hold his job. He should be expected to see the importance of the great thresholds of scientific discovery and of humanistic interpretation and he should be eager to cross them and feel the excitement of the search.—*From the Annual Report (for 1943) to the Trustees of The Johns Hopkins University by President Isaiah Bowman, predecessor of Dr. Carlson as President of the A.A.A.S.*

PRECIPITATION AND STREAM FLOW

By CLARENCE S. JARVIS

LONG before the introduction of systematic snow surveys, irrigation farmers had undertaken the appraisal of their prospects for water supply by noting the snow accumulations on mountain passes and on the slopes visible from the valleys. Actual rainfall records, including all forms of precipitation, together with well-organized snow surveys among the headwaters areas, now furnish the essential information for making the planted acreage proportional to the prospective water supply.

In some instances glacier-fed rivers have shown apparent disregard for the current season's snowfall. For example, in a year of drought the more abundant sunshine should insure greater melting of the ice surface, thus compensating in some degree for the deficient snow cover elsewhere. The almost universal trend among glaciers observed in recent years seems to be recession of several feet per decade. This may indicate either a decrease of precipitation or an increase of sunshine, or possibly both of these tendencies as compared with long-period averages heretofore.

In general, stream flow represents some fraction of the precipitation of the current year or season, together with some of the storage accumulated in previous months or even years. Some hydrologists prefer to regard the runoff as a residue, after the various losses have been subtracted from precipitation in all of its various forms. Assuming that the precipitation is known, and also the temperature, humidity, wind movement, and hours of sunshine, a fairly satisfactory estimate may be ventured regarding evaporation from free water surfaces, from the soil, from foliage and other surfaces of vegetation by transpiration, and its counterpart from ice or snow surfaces by which the moisture is transferred to the atmosphere through sublimation. After a few hours of drying winds, a snow or ice surface may become honeycombed, as a preliminary to its complete absorption into the atmosphere.

Mississippi valley rainfall and runoff. It has been officially determined by the United States Weather Bureau that the average annual precipitation over the Mississippi River drainage basin is 30.1 inches, and it has been disclosed by the publications of other Federal agencies that the average discharge, or surface runoff, amounts to approximately 6.9 inches over the entire basin, or 23 per cent of the rainfall in all its forms. The remaining 77 per cent evidently returns to the ocean or escapes from the Mississippi Valley mainly as atmospheric moisture, and in minor amounts as seeps and springs.

Evaluation of this fundamental datum of annual runoff depth for the average year is but a beginning in the study of stream discharge. The wide variations in stage and flow from extreme drought conditions to high flood, or from month to month and from day to day, all are important and predictable in varying degrees, as has been demonstrated by many years of notable flood forecasting and associated hydrologic service. Expressed in terms of the past 70-year mean discharge, the maximum and minimum values for the Mississippi River at or near its mouths would be 2,440,000 and 102,000 cubic feet per second (c.f.s.), as compared with a mean of 635,000 c.f.s. It should be recalled that more than three times the mean discharge is escaping in the meanwhile to the ocean or to adjoining drainage basins by atmospheric movement following evaporation or through underground channels.

Probably the increased opportunity for evaporation afforded by irrigation and other water utilization projects, including their great storage reservoirs and canal systems, may represent about 4 per cent of the mean discharge at the Mississippi River mouths, or nearly 1 per cent of the total precipitation. This subtraction from river discharge only partially compensates for the increased runoff due to the drainage of natural lakes and swamps, the clearing of forested areas, and the cultivation of meadow

or range lands, with attendant soil erosion, and gullying; all of which aggravate the flood menace.

Variability of discharge. In view of the dearth of actual records on many of the great river systems, and also because of the difficulties encountered in obtaining such fragmentary data as have been assembled in many foreign languages, the discharge habits of many obscure river systems have remained enshrouded in mystery. Recent accessions of actual discharge records, however, have supplied the required basic information on which to compute, estimate, or interpolate preliminary values of some of the hitherto missing quantities. One of the disturbing influences is the widely varying duration of records, with different terminal dates. Consequently the means thus provided may reflect periods in which either subnormal or excessive rainfall occurred.

Table 1 shows the variability of discharge

TABLE 1
CHARACTERISTICS OF SELECTED RIVERS

River	Mean annual—		Runoff factor	Variability of discharge
	Precipitation (P)	Runoff (R)		
	Inches	*Inches*	*R/P*	*Max./Min.*
Amazon	64	19	30	9
Colorado	13.0	1.3	10	438
Columbia	29.0	14.4	50	20
Mo.-Miss.	30.1	6.9	23	24
Nile	32	1.2	3.7	160
Ohio	44.2	18.3	41	75
St. Lawrence	35.0	12.5	36	6
Tennessee	50.0	22.9	46	97
Volga	17	3.8	22	60
Yukon	14.0	9.0	64	25

for selected river systems; that is, the maximum divided by the minimum discharge. Obviously, this method of comparison would not serve for rivers that go dry, as the quotient of any integer divided by zero would be infinitely large. Owing to storage of its water in the Great Lakes, the St. Lawrence shows a variation of only 6 between maximum and minimum discharge in contrast with slightly more than 1,000, 2,000, and 3,000, respectively, for the Potomac, Humboldt, and Brazos Rivers.

If undisturbed by man, the flow from a typical mountain brook at the edge of a desert region may extend a considerable distance into the arid basin in the early morning and to lesser distances as the evaporation increases with the heat of the day. This illustrates on a small scale some of the changes to which large river systems are subject along their long, tortuous courses. Thus the drainage area of the Nile has a mean annual rainfall of about 32 inches, but the river actually delivers only 1.2 inches to the lower delta region, or barely 4 per cent of the rainfall. However, if the measurements were confined to the headwaters astride the equator, the rainfall would be found to exceed 60 inches, and the runoff would probably reach at least 30 inches because the water rushes down steep mountain slopes of Kilamanjaro and other mountain peaks rising to elevations of three miles or more above sea level. A higher runoff would be prevented by transpiration from the dense jungle growth and by evaporation from storage water in Lakes Victoria and Albert, as well as from extensive marshes along the river channel. That modest 4 per cent of the annual rainfall, which habitually caused the Nile to overflow and irrigate the rich valley and delta lands, practically always insured abundant harvests, so that it could be said generally throughout the centuries, "There is corn in Egypt."

As for the other 30.7 inches of moisture supplied by rainfall over the broad expanse of the Nile River basin—only about 10 per cent less than the Mississippi Basin in area—who can say that it represents absolute loss? How many of the oases are supplied by the underflow from valley fill or from stratified rock formations? How many of the freshening showers of Eastern Africa, Arabia, and Palestine originated in Nile Valley evaporation? And how about the bountiful forest and agricultural products of the Upper Nile basin?

Comparison of outstanding river systems. In the higher latitudes, where annual precipitation is mainly in the form of snow and is usually limited to equivalent rainfall of 16 inches or less, except for mountainous country, the perpetually frozen subsoil pre-

cludes loss into deep formations, and the reduced evaporation limits losses into the atmosphere, thus increasing the runoff factor. For such conditions, the runoff will generally represent 30 to 60 per cent of the precipitation, depending on local factors such as summer temperatures, humidity, wind movement, and exposed free water surface. In contrast, from many of the river basins in equatorial regions less than 30 per cent of the rainfall runs off, because the dense jungle growth returns as much moisture to the atmosphere as would a free water surface, or occasionally more. Who can deny the suggestion that the successive afternoon torrential downpours of the tropics merely precipitate the transpiration and evaporation of the previous day in neighboring jungles?

Determination of drainage areas naturally requires the use and interpretation of the best available maps. The main channel length necessarily includes the midstream distances around bends of both the main river channel and also the principal tributary among the headwaters, even though they may have different names. The lengths compared are those of the channels along the main stems of the river systems, from source to mouth. The longest river channel is the Missouri-Mississippi, with the Amazon and Nile coming next in order, each 4,000 miles or more. The mouth of rivers flowing into the ocean is to be regarded as a point near, but not in, an estuary.

One of the familiar items of elementary school geography has to do with the Orinoco headwaters, one portion turning southward into the Rio Negro, a tributary of the Amazon, thus making an inland waterway connecting the two river systems, particularly during the season of high water. Similarly, but in somewhat less degree, light draft boats may pass from the headwaters of the Guapore branch of the Amazon's great Madeira River to the Paraguay River system.

The La Plata River is essentially an estuary, disturbed by tidal movements throughout its 200-mile length. The discharge of the La Plata is taken as the summation of discharges from its tributaries, such as the Uruguay and the Parana, including the Paraguay. Naturally, the length of channel includes that of the Parana to its headwaters on the slopes of a mountain almost within view of the heights overlooking Rio de Janeiro. The Iguassu or Curitaba River, a tributary of the Parana, plunges over falls much higher (210 feet) than those of Niagara, with considerably greater flood volume, but probably lower mean flow.

Value of fragmentary data. Where the basic data are scanty, incomplete, or of uncertain quality, as for the inland basin draining into the saline Sea of Aral, a valuable clue or check is afforded by equating the evaporation from the expanse of free water surface against the runoff from the mountainous rim and increasingly arid plains approaching the saline sink. If the total drainage area is 40 times the area of the inland sea, and if the average annual evaporation for the globe is applicable, say about 40 inches, then the runoff from the total drainage area would obviously be one inch in depth for the same period. But for the Sea of Aral, about 50 degrees North Latitude, and with climatic characteristics, especially temperature, somewhere between those of Tashkend and Turgai, U.S.S.R., in what was formerly Turkestan, it is probable that somewhat less than the average evaporation rates for the globe would prevail there, and that the runoff would consequently be somewhat less than one inch. Similarly, evaporation from the Dead Sea of Palestine, with its surface nearly a quarter of a mile below sea-level and with subtropical temperature and aridity generally prevailing, must be of the order of 50 inches per year; and this agrees with the volume of runoff, 3.7 inches, as determined for this basin, of which the Jordan River is the chief contributor.

Uncertainty attaches to the estimates of mean annual precipitation over large drainage basins, even where numerous station records are available, inasmuch as the remote or thinly settled areas, such as jungles and high mountain slopes, possibly receiving the heaviest rainfall, may have inadequate representation among the basic records. Furthermore, the occasional gagings of remote rivers and tributaries may depart widely from the long-period means.

Hardly a season or a year passes without the establishment of new records in high or

low rainfall and associated river discharge, in various parts of every continent. It is well known that subnormal rainfall in two or more successive years aggravates the shortage of runoff, because the underground reservoir has been depleted and not refilled to its customary level. Conversely, successive years of excessive rainfall not only refill the usual underground storage space, but also develop new ones, so that the high or low river discharge of a series of abnormal years must be explained partly with reference to the year or two preceding. An outstanding example is the Mississippi River flood of April 1927, which was actually forming and giving cause for concern, if not alarm, months before its culmination. Fortunately, the Federal agencies as well as the local municipalities whose duties include provision against catastrophes associated with excessive river flow or with severe shortage have taken to heart the lessons of the past and have endeavored to make ample provision against recurrence of such happenings without forewarnings to all concerned. In such emergencies they have to integrate the fragmentary data as they become available and then draw conclusions based largely on previous experience under conditions approaching those under observation. It is remarkable how their forecasts are usually borne out, several hours or even days later.

Mean annual precipitation and evaporation over the globe. A coincidence worth noting is that both the evaporation from a free water surface and the precipitation for the average year at Washington, D. C., are about 40 inches, or nearly the mean for the entire globe. In high latitudes as well as at high altitudes, the total annual evaporation in all its forms may be as low as 6 inches, largely due to the low temperature and to the correspondingly restricted moisture capacity of the atmosphere. In desert regions near the tropics, the annual evaporation may exceed 80 inches from a free water surface, particularly for shallow depths that may be warmed each day of bright sunshine.

Under desert conditions the free water surface of river systems is either reduced in area or else subjected to heavy toll. From each occasional shower the sandy soil absorbs a generous portion for storage at depth while the surface dries and provides the needed insulation. Only by reason of the small area of free water surface relative to the total drainage area, can a river like the Nile, the Niger, the Colorado, or the Rio Grande deliver any considerable volume after meandering nearly 1,000 miles through country of such pronounced aridity that water lost by evaporation exceeds by four or more times the actual rainfall of the lower valley.

Storage losses. The penalty paid for regulation of the Rio Grande by storage in Elephant Butte Reservoir amounts to some 20 per cent of the mean flow, and it is well worth while at that price. Similarly, Nile River regulation through storage at Aswan probably exacts a toll of about 6 per cent. However, the total storage from the Aswan Reservoir to the headwaters in reedy swamps, natural lakes, or recently constructed reservoirs permits evaporation of possibly double or treble the volume that now reaches Aswan for use in the delta regions. No one can reasonably deny that the 4 per cent of the rainfall regulated at Aswan is more valuable than would be 16 per cent of unregulated, highly variable discharge that would endanger the economy of the entire alluvial valley, along with life as well.

The great depth of Lake Mead above Boulder Dam on the Colorado River is one of the factors accountable for keeping the storage losses in that reservoir at somewhat less than 10 per cent of the inflow. If the regulation had cost 40 per cent instead, it would still have been worth while, for undoubtedly some of it would return as rainfall to the same basin.

After observing the general inundation along the lower Potomac River and over Potomac Park, including some residential and business districts in March 1936, who would have imagined that the flow would ever diminish to such a low stage as to threaten shortage in the municipal water supply for the District of Columbia and adjoining metropolitan areas? It is generally known and accepted that adequate storage for regulation of the Potomac River can correct both situations.

An important factor in storage and chan-

nel loss is infiltration of water into the sedimentary deposits of bed and banks, thence to emerge at lower elevations in either the same valley or neighboring basins and to supply seeps, springs, water tables within reach of roots, or even outcropping at some oasis or depression. Such underground storage and distribution systems, under pressure in artesian areas and largely exempt from the usual losses to which surface streams and storage are subjected, have thus far been traced, utilized, and appreciated only in part; but what rich dividends the geologists have brought forth from this field!

Rivers of Greenland. The Greenland Icecap may be regarded as the principal river of that island-continent, with mouths or outlets leading down to the coast in all directions. Under the pent-up pressure developed by hundreds or perhaps thousands of feet of ice and snow column, the cap yields to plastic flow; and when the overhang is sufficient, end-slices of the mammoth layer-cake are sheared off by their own weight into the sea. Occasionally the intact carcass of some form of life is disclosed as the icebergs melt; but the chill induced in the neighborhood of these floating masses, and the associated fogs that add further hazards to navigation and observation, have kept such discoveries as very rare happenings. Therefore many a page of natural history has been turned but to melt unseen and likely never to be read. If the runoff from Greenland only keeps pace with the mean annual precipitation, it is probably nearly 12 inches per year, while evaporation and sublimation together would account for nearly 6 inches; but no one has determined whether the ice-accumulation reserves of past ages are being either augmented or overdrawn during the average year.

Wherever the mountain elevations and precipitation are sufficient, icecaps in miniature are to be found, even near the equator, from the southern island of New Zealand to the principal mountain ranges of the Americas, and from the Himalayas to the Alps, providing very important and valuable adjuncts to the regulatory storage, and capable of functioning apparently as well in years of drought as in seasons of plenty, with volume of yield and time of delivery fairly predictable months in advance, and therefore encouraging comprehensive planning for complete utilization. And as to the river systems with spring sources on the plains and lowlands, how many of them depend upon high mountain basins, connected by underground channels, even though they be either the joints and planes of stratification or porous sedimentary rocks, yielding to enormous water pressures from highlands?

TREES OF SOUTH FLORIDA*

II. FIVE NATIVE CABINET WOODS

By JOHN C. GIFFORD

Woods that are regarded as suitable for the construction of fine household furniture are called cabinet woods. Such woods are generally characterized by beauty of finished grain, by strength and hardness, and by adaptability to carving. These are the characteristics of heartwood from the five species of trees to be described in this article: lysiloma, mahogany, fishpoison-tree, false-mastic, and lignumvitae.

In South Florida these trees are found in wooded areas that Floridians call hammocks, a name, probably of Indian origin, that should not be confused with hummock; although sometimes a hammock island in a prairie looks like a hummock or small hill. The word hammock refers to a mixed growth of hardwood trees occurring in tropical countries wherever the annual rainfall is about fifty inches. Sometimes palms are included, as in Royal Palm Park near Redlands, and further north there are many cabbage-palm areas, which are also called hammocks.

I do not call attention to these five cabinet woods for the purpose of establishing woodworking industries. An outside market for the wood is not needed since all that we have has been, and can be, used locally. The sole purpose of this paper is to direct attention to the fact that these woods are native, that they can be grown easily in South Florida, and that the trees now remaining should be protected to prevent their extermination.

Lysiloma

Lysiloma, like casuarina, is a generic name which, at least among foresters, has become the common name of *Lysiloma bahamensis* Benth. Foresters object to the more prevalent name "wild-tamarind" because lysiloma is not a tamarind. But by whatever name it is called, it yields a valuable cabinet wood, combining the virtues of black walnut and mahogany. The wood of lysiloma is heavy, hard, tough, close-grained, rich dark brown tinged with red, and with a white sapwood. It is occasionally used and valued for shipbuilding, and a wood that is used for shipbuilding must be good.

Like lebbek, described in Part I, lysiloma is a leguminous tree. Its foliage is light and fernlike and is often a beautiful reddish color like that of the mango and other tropical leafage when young and tender. Lysiloma bears white flowers, about one-third of an inch long, in dense heads. The fruit is a thin and papery pod containing many dark brown "beans" about half an inch in length.

FEATHERY LEAVES OF LYSILOMA

Although limited in its distribution, lysiloma is one of the hardiest trees in South Florida. It has withstood fire and hurricane better than many other species. A heavy seeder even when young, it would, if encouraged, soon cover our rocky inland keys and our shore keys with a rich mantle of green.

Lysiloma not only provides a wood of beauty and durability, but is useful, like lebbek, for increasing soil fertility through

* Continued from p. 28 of the preceding issue.

the fixation of atmospheric nitrogen by the bacteria on its roots. Mahogany would grow in association with lysiloma and would be benefited by it. Lysiloma could also supply shade needed in tropical agriculture. In spite of its many virtues, lysiloma has attracted little attention and all the economic literature relating to it would hardly fill a printed book page.

There were once some very large lysiloma trees in a hammock in Miami—now Simpson Park—and they are still quite common in the hammocks of the Redland District and on the keys. Fortunate as we are in having this rare, majestic, and useful tree in our hammocks, it seems strange that we have never appreciated it; on the contrary, we have cut and burned it as if it were a useless weed. Many logs of this precious wood have rotted in the forest and some big trees, killed by fire or girdled to make way for the growing of truck crops, are still standing although long dead and bare. On the keys, where hardwood forests were destroyed long ago to clear land for growing tomatoes (they could have been grown on prairie land instead), young lysilomas are spreading by the thousands to repair the damage.

Almost every tropical country has a leguminous tree that it fosters and cherishes. The lysiloma is still waiting to be discovered in South Florida.

Mahogany

Floridian mahogany is now in such great demand for furniture and boat construction that every acre of wild land is being searched for trees. Airplane reconnaissance is now being used to spot the big trees in the dense hammock islands in the mangrove swamps.

Unknown to the public is a spot in South Florida where a group of virgin mahoganies is surrounded by swamp. These trees are three or more feet in diameter, breast high.

A GROUP OF LYSILOMA TREES SHOWING THE WHITE BLOOM

A BRANCH OF MAHOGANY SHOWING LEAVES, FRUIT, AND SEEDS
THE SEEDS (RIGHT) ARE WINGED LIKE THOSE OF THE MAPLE, ROTATING AND DISPERSING AS THEY FALL.

They are broad-spreading with humus a foot or more in depth beneath. Living in the surface duff are rats, which no doubt feed on the seeds of the mahogany and false-mastic trees, and also on tree snails. Rattlesnakes in turn feed upon the rats. Lysilomas and fishpoison-trees are also present. Perhaps there are other such spots that might be preserved to show this and coming generations what once was widespread. We fail to realize that the landscape is constantly changing through man's interference, and what some of us saw in the Florida of old will never be seen again.

Mahogany is so well and favorably known that many other woods have taken its name in vain. It has been hard to convince people that the genuine, old-time mahogany is native to Florida; that it was, in fact, common at one time here. Botanists have known this for many years. In the winter of 1942–43 unusual cold weather killed unprotected vegetables, yet the mahogany was not injured, indicating that it might be grown even farther north than its present range.

The mahogany of Florida (*Swietenia mahagoni* Jacq.) is heavier than the mahogany of Mexico and Central America (*Swietenia macrophylla*), and the former is superior for the manufacture of slender parts of furniture, such as the legs of chairs. It has been used for many years for the rails of boats and of late in the manufacture of very solid revolving chairs for deep-sea fishing. The rich, reddish brown Floridian mahogany is often specked with small black spots which disappear to some extent as the wood darkens with age. For many people the beauty of the wood is enhanced by these spots.

The flowers of the mahogany are inconspicuous, yet they ultimately produce pods the size of turkeys' eggs. These open from the bottom, shedding many winged seeds, which look like the seeds of a maple tree. Of all our trees, few are greater seed-producers and few can germinate and grow on rough, rocky soil as freely as the native mahogany. The winged seeds, rotating as they fall, are so retarded in their descent that they are widely spread by the wind. As cattle do not eat the bitter foliage of mahogany, it tends to gain ground in pastures.

The mahogany tree is not so slow to grow as is ordinarily supposed. Hard, heavy wood does not necessarily mean slow growth. The northern black locust is a hard and heavy

wood and yet it is one of our quickest growers. I have known mahogany on mediocre soil in Florida close by the sea to add one inch in diameter each year. In rich, moist soil it is a fast grower with a broad spread of dense foliage. When growing in an uncrowded location, it has massive limbs. These limbs are unexcelled for the ribs of boats as well as for furniture, because a beautiful crotch grain occurs where the limbs join the trunk.

Mahogany is a yardstick for other woods, and the tree in turn compares favorably with the live oak for grace and sturdiness. After a recent cold snap some of the mahoganies on the mainland dropped their leaves. There soon followed a fine crop of pale green leaves, some tinged with red. This interruption in growth probably produced a distinct ring in the wood which would give it a variety of grain lacking in wood further south where growth is continuous.

Fishpoison-Tree

The fishpoison-tree, *Piscidia communis* (Blake) Harms, is more commonly known as the Jamaica-dogwood, but botanists reject the latter name because it is misleading.

LEAVES OF THE FISHPOISON-TREE

Piscidia communis in no way resembles the well-known flowering dogwood, *Cornus florida*. Moreover, it does not really belong to Jamaica, since it is one of our commonest hammock trees and is found also in Yucatán. Trees were often named for the uses of the wood. As various pieces of wood used in implements and machines were called "dogs," any wood, regardless of botanical origin, that might be used as a "dog" could be called dogwood. However, botanists now prefer to restrict "dogwood" to species of *Cornus*.

As the name indicates, the fishpoison-tree has been used principally to stupify fish in order to catch them easily. The roots of a number of other tropical leguminous trees and shrubs have also been used for this purpose throughout the world. The bark of the root is pounded into a pulp, put into a basket, and lowered into a fish hole. Sometimes it is merely thrown into the water. The fish soon come to the surface, inactivated by certain toxic substances washed from the bark. This practice was probably learned from the Indians. It does not kill the large fish but is fatal to the small ones. Although some fish-poisoning plants, such as derris and cube, have become important as sources of insecticides, the fishpoison-tree has not shown promise for this purpose.

The fishpoison-tree was much used years ago for timbers in boat construction. Once, on the keys, two brothers wanted a new boat. They had settled on Pensacola yellow pine for the planking, but could not agree on the kind of wood for the ribs. One insisted on "madeira," the Florida name for mahogany; the other insisted on "dogwood." I acted as mediator, and it was finally decided to put the ribs close together, alternating mahogany and "dogwood." Although that was twenty-five years ago, the boat still floats, and all the ribs are still solid. The fishpoison-tree will be important someday in the forestry of the tropics. The fact that it has been used for the ribs of boats, the fellies of wheels, and the frames of vehicles speaks for its strength and durability.

The fishpoison-tree has luxuriant leafage and bears masses of small, pea-like, white flowers tinged with red. The fruit consists of broad, four-winged, light-brown pods.

A FALSE-MASTIC TREE TOWERING IN MATHESON HAMMOCK

Regeneration is good in the regions where it is native and many seedlings are to be found in spite of cuttings and fires. There are two distinct forms of this tree in South Florida, hardly different enough to warrant the formation of another species.

False-mastic

False-mastic (*Sideroxylon foetidissimum* Jacq.), like lignumvitae, yields a wood that is heavy enough to sink in water. The generic name means ironwood. "Mastic" in the common name indicates that this tree also yields a gum, which is found particularly in the fruits. The yellowish or orange wood of the false-mastic is hard, strong, and durable.

False-mastic grows in abundance in Snapper Creek Hammock and other hammocks in South Florida. I called these trees to the attention of Mr. W. J. Matheson. He was so much surprised and elated at the size of these "mastic" trees that he bought the land and willed it to the county. It is now a popular park known as Matheson Hammock.

The flowers of false-mastic are inconspicuous. The fruit, which is usually abundant in the spring, is about an inch long with a tough skin and thin flesh covering a seed about a half-inch in length. It is eaten by various animals but is not fit for human consumption. The bark is dark gray, sometimes light brown, forming in plate-like scales. The tree is usually tall and straight.

A HAMMOCK PATH, SIMPSON PARK
THE YOUNG TREES ON THE RIGHT ARE LIGNUMVITAE.

False-mastic extends northward on the east coast of Florida as far as Cape Canaveral and is common in Cuba and the Bahama Islands. This tree, in fact all trees here mentioned, are such good seed producers that any person desiring to produce these trees in quantity would have no trouble in finding seeds locally.

Lignumvitae

Lignumvitae, the "wood of life," is so called because at one time the gum in its wood was considered one of the most valuable medicines of the world. The name of this resin, guaiacum, is also the generic name of lignumvitae (*Guaiacum sanctum* L.). Guaiacum is obtained by soaking the sawdust and chips in alcohol or ether. Tears of the resin exude from wounds on the living tree.

With the probable exception of leadwood or "Florida ironwood," *Krugiodendron ferreum* (Vahl) Urban, lignumvitae is the heaviest wood growing in the United States. It weighs about 80 pounds per cubic foot and has a specific gravity of 1.32.

One of the Florida keys is called Lignum Vitae Key because this tree was once plentiful there. I have found it on Big Pine Key, Key Largo, and other keys, but this interesting and valuable tree is rapidly passing. In the battle against axe and fire any tree must produce an immense amount of seed in order to survive. In spite of a constant demand, however, a tree now and then may be seen in secluded places. The wood is still sold by the pound and is probably the most valuable of all commercial woods. Because of its toughness there is probably no wood with a greater variety of special uses. Its earliest use was in the manufacture of sheaths for ship-blocks. In the days of sailing vessels these blocks were essential, and for many years the woods by tidewater in Florida were used for ship construction by at least three nations, not to mention pirates and other outlaws. Of all the special parts of a ship demanding special woods, none was more important than the ship's blocks, through which the ropes could run easily without the danger of

breaking under the great strain. Later in the days of steamships lignumvitae was used as bushings for lining the tubes of propeller shafts—a most important and exacting application. Today it serves for mallets, tenpin balls, castors for furniture, brush backs, and a host of similar uses. Its wood fibers are interlaced in such a way that it never splits.

The tree is short and stout. It occurs in the shade in mixture with other plants. It is of slow growth and in some places looks as tough as its wood. It is often fresh green when other plants have been seared by drought. Three or four flowers under an inch in diameter are produced at the ends of the branches. The color of these delicate flowers varies in shades of blue. The fruit is roundish, less than an inch in diameter, and is bright orange when ripe. Inside are black seeds covered with a scarlet coat.

As a plant for future landscape architecture and forestry in the tropics, it probably should have an important place. In the West Indies many lignumvitae trees persist, but in the State of Florida it will soon be a thing of the past unless preserved by those who are interested in useful native trees.

Treatment of Cabinet Woods

None of the foregoing cabinet woods should ever be painted or even stained, for their beauty is in their natural grain and color. The surface of the wood should be made as smooth as possible and then, if covered at all, should be coated with some transparent substance. "Elbow grease" is, after all, the best. Good seasoning is necessary, though not always understood even by experienced workers of wood. Sometimes the natives bury the wood in mud for several weeks to season by a process of osmosis, and then keep it for a long time in a dry, shady, airy place. The heartwood of old trees does not need much seasoning.

The ends of logs in piles are often painted to prevent too rapid evaporation. Planks of such wood are usually set up loosely on end against a wall and against each other to facilitate seasoning. This practice exposes all the wood to the air and facilitates even drying. A change from a warm to a cold climate often produces many small cracks in the wood, especially when the wood has not been perfectly seasoned. Many cabinet-workers have special methods for preparing woods for their particular work, but on the whole the main object seems to be to produce a slow, even process of seasoning over a considerable length of time. Wood dug out of the mud and muck in which it has been buried for many years is perfectly seasoned and very durable. Many of the old logs of mahogany and lysiloma buried in the debris of the forest are perfectly seasoned. In virgin forests where there have been windfalls and no fires, there are many such fallen trunks. In such cases it is usually only the heartwood that has escaped the termites. Many mahogany trees were cut from a scaffold far above the bulge at the base and in these old stumps there is still much well-seasoned wood with beautiful grain.

For many years private interests and government bureaus have been busy importing species of trees from abroad. This has been good work, and the results have warranted the cost and effort involved. Several of these introduced trees have become naturalized. Experimentation along this line, however, must cover a long period of time before conclusions are warranted. In the meantime we have neglected some of our native trees. Nature has done this experimentation for us. For that reason the forester should pick the best of our native trees, study their natural forest formation before it is too late, and proceed to apply the fundamental rules of forestry to what is left as soon as possible. For this purpose the five species mentioned in this article are unquestionably of great merit.

In some parts of the world where species of plants have become exceedingly rare, laws have been passed preventing their ruthless destruction, regardless of their location or ownership. This rule is applied even in this country to certain rare animals. Man gains title to wild land in various ways; he then builds fences and kills off whatever seems to oppose him. This so-called progress has sometimes resulted in a sacking of our inheritance. There is a vast difference between exploitation and true development.

CUTANEOUS LEISHMANIASIS IN MEXICO*

By ENRIQUE BELTRÁN

An examination of the figures on pre-Spanish pottery found in certain parts of Peru reveals some very peculiar scars on a number of the human faces represented on them. These scars indicate the existence of a disease of ulcerous nature suffered by the ancient inhabitants of the powerful Inca Empire.

Such authorities as Professor Brumpt think that these pictures are unequivocal proof that there already existed in those distant times the disease known today in Peru as *uta* and *espundia*, in Brazil as "Baurú ulcer" or *ferida brava*, and in Mexico as "chicle-gatherers' ulcer." Apparently the northern limit of its geographical distribution is reached in Mexico.

If the disease itself is ancient, its characterization as a definite nosological entity and the clarification of its etiology are quite recent. Only in 1909 did Lindenberg, and Carini and Paranhos, all in Brazil, identify the "Baurú ulcer" or *ferida brava* with the so-called "Oriental button" or "Oriental ulcer" occurring in the hot, dry regions of the Old World, especially in the Mediterranean basin. Since the beginning of the century, a protozoan of the genus Mastigophora, named *Leishmania tropica* (Wright), 1903, has been considered the cause of the latter disease.

Those who first investigated it in the New World believed that the American disease was the same as that already known in Europe, Asia, and Africa; consequently, they supposed that the parasite found in the lesions was the same *Leishmania tropica*. In fact, the parasites of both are so similar that their morphological differentiation is impossible. Nevertheless, Vianna, a Brazilian investigator, in consideration of the marked clinical differences observed in the American disease as compared with its course in the other hemisphere, believed it desirable to regard the Brazilian parasite as a new species, which, in 1911, he named *Leishmania brasiliensis*. This designation is now accepted by the majority of investigators who believe that they are dealing with a distinct species—a view reinforced by the studies on immunology carried out by Noguchi.

In that same year, 1911, Seidelin discovered cutaneous leishmaniasis in Mexico, while studying yellow fever in Yucatán, and published the first description of it that we know of from that country. It is reproduced here for its historical interest.

> I had, for a long time, been on the look out for the various forms of tropical ulcers, but failed to find any of a specific nature, until Mr. Th. Maler, an Austrian archaeologist, who has lived for nearly fifty years in Yucatán, told me about a peculiar affection which he had observed with great frequency during his travels in the interior. He said that the "chicleros," Indian labourers, who collect chicle (the raw material of chewing gum), suffered severely from a certain kind of ulcer, which always started on the external ear, often destroying it completely, and invading the face to a large extent. The natives did not, as a rule, apply for treatment, and even if they did, favourable results were but seldom obtained. . . . The disease is evidently met with in all parts of the peninsula, both in the interior of Yucatán, in Campeche and in Quintana Roo. Also many laymen knew the affection as a typical one, the "ear ulcer of the chicleros." . . . The parasites observed had the morphological characters of *Leishmania tropica;* they were mostly situated inside large mononuclear cells, from two or three to about twenty in a single cell.

More than thirty years have passed since this disease was made known in Mexico, but we are still ignorant of many of its aspects, despite the diverse but hardly numerous studies which have been made of it.

In the forested areas which account for two-thirds of the Yucatán peninsula one of the chief resources is the tree that is popularly called *chico zapote* (*Achras zapota*).

* Translated by Mr. Hurst K. Majors. The original Spanish text is published opposite on right hand pages. The illustrations are distributed between the Spanish and English texts. Grateful acknowledgment is made to Dr. A. A. Moll, Sanitary Bureau, Pan American Union, Washington, D. C., for his careful editorial examination of both the Spanish and English texts.

LA LEISHMANIASIS FORESTAL CUTÁNEA EN MÉXICO

POR ENRIQUE BELTRÁN

Examinando las figuras de cerámica prehispánica encontradas en ciertas partes del Perú, suelen verse en algunas de las caras humanas en ellas representadas, cicatrices muy peculiares, que indican la existencia de un padecimiento de naturaleza ulcerosa, en esos remotos habitantes del poderoso imperio de los Incas.

Autoridades, como el Profesor Brumpt (*1*), piensan que esas representaciones son pruebas inequívocas de que, en aquellos lejanos tiempos, existía ya la enfermedad hoy designada como "uta" y "espundia" en Perú, "úlcera de Baurú" o "ferida brava" en Brasil y "úlcera de los chicleros" en México que, al parecer, constituye el límite Norte de su distribución geográfica.

Pero si la existencia de la enfermedad es antigua, su caracterización como una entidad nosológica determinada y, sobre todo, el esclarecimiento de su etiología, son bastante recientes. Fué en 1909 cuando Lindenberg (*2*) por una parte, y Carini y Paranhos (*3*) por otra, todos en Brasil, identificaron la "úlcera de Baurú" o "ferida brava" con el llamado "botón de Oriente" o "úlcera oriental," existente en las regiones cálidas y secas del Viejo Mundo, especialmente en la cuenca del Mediterráneo y que, desde principios del siglo se consideraba causada por un protozoario del grupo de los Mastigóforos, cuya designación en la actualidad es *Leishmania tropica* (Wright), 1903.

Los primeros investigadores del Nuevo Continente pensaron que la enfermedad americana era la ya conocida en Europa, Asia y Africa y, en consecuencia, se supuso que el parásito encontrado en las lesiones era la misma *Leishmania tropica*. Y en efecto, los parásitos que se presentan en uno y otro casos son tan semejantes, que su diferenciación morfológica se hace imposible. Sin embargo, un investigador brasileño, Vianna, considerando las marcadas diferencias clínicas que se observan en el padecimiento del Nuevo Continente, comparado con el del Antiguo, pensó prudente individualizar el germen encontrado en Brasil, designándolo con el nombre de *Leishmania brasiliensis*, en el año de 1911; designación aceptada en la actualidad por la mayoría de los autores que piensan que, en efecto, se trata de una especie distinta, particularmente después de los estudios inmunológicos llevados a cabo por Noguchi (*4*).

En el propio año de 1911 Seidelin (*5*), estudiando la fiebre amarilla en Yucatán, México, encontró la leishmaniasis cutánea en nuestro país, y publicó la primera descripción de la misma de que tenemos noticia y que, por su interés histórico, cito *in extenso* a continuación:

> Por largo tiempo había estado buscando las varias formas de úlceras tropicales, sin encontrar ninguna de naturaleza específica hasta que el Sr. Th. Maler, arqueólogo austriaco que había vivido en Yucatán por cerca de cincuenta años, me habló de una enfermedad que había encontrado con gran frecuencia durante sus viajes en el interior. Decía que los "chicleros," trabajadores indígenas que colectan chicle (la materia prima de la goma de mascar) sufrían severamente de una cierta clase de úlcera, que siempre comenzaba en la oreja, destruyéndola a veces completamente, e invadiendo ampliamente la cara. Generalmente los nativos no buscaban tratamiento, y cuando lo hacían, solo rara vez obtenían resultados favorables. . . . La enfermedad se encuentra, evidentemente, en toda la península, tanto en el interior de Yucatán, como en Campeche y Quintana Roo. Aun los profanos conocen la enfermedad como típica: la "úlcera de la oreja de los chicleros." . . . Los parásitos observados tienen las características morfológicas de *Leishmania tropica;* se encuentran en su mayor parte dentro de los grandes mononucleares, en número desde dos o tres, hasta veinte en una sola célula.

Hace, pues, más de treinta años que se señaló esta interesante enfermedad en nuestro país y, sin embargo, hasta la fecha ignoramos aun muchos aspectos de ella, a pesar de las diversas contribuciones realizadas al respecto, no muy numerosas por cierto.

Foto Robles

CHICLE-GATHERERS' ULCER

LESION PRIMARIA, EN LA NARIZ, CAUSADA POR *Leishmania brasiliensis*, SIN TRATAMIENTO.

the sap of which, chicle, is the basic material used in the manufacture of chewing gum, a popular article in the United States and Mexico. The English common name of this tree is sapodilla.

Some remnants of the Mayan tribes, who populated the peninsula during ancient times and who constructed the great cities of Uxmal, Chichen Itza, Tulum, and others, live in the midst of these forests even yet as small groups. However, the number of permanent inhabitants is very low throughout the entire region.

The jungle population is made up for the most part of workers who contract to secure chicle or to cut down hardwoods. They are adventurers who bury themselves in the green hell of the jungle for months in order to seize its treasures.

The *chicleros*—some of them natives of Yucatán and others from other parts of Mexico—begin to penetrate the jungle at the close of the dry season. In the past they advanced in caravans to the heart of the forest along paths which had to be kept open with the machete; today, for the most part, they reach their jungle centers by air and then continue on foot in small groups to seek sapodilla trees in the forest.

From our own investigations, which began in 1939, we have learned that of these groups of workers, mostly men but sometimes including wives and children, approximately 5 per cent end the season affected by forest leishmaniasis of the skin, the fearsome *chiclero* ulcer.

In South America, and especially in Brazil where most of the work on this disease has been done, the disease begins in an almost imperceptible fashion. An individual finds on one of the exposed parts of his body (in Brazil, usually the legs) a small reddened area, similar to that which results from the bite of one of the many jungle insects; the fierce itching which he experiences forces him to scratch himself repeatedly. Gradually a small papule is formed, and then a little nodule, the surface of which soon ulcerates, forming a scab. Then *Leishmania* with the help of other associated infections rapidly destroy the tissues. When the lesions occur on the legs, arms, chest, or back, the course of the affliction, although troublesome and destructive, is without serious consequences. It is different if the ulcers occur on the face. Not only is a deforming scar left, but a considerable number of cases experience an infection of the mucous membranes of the nose and mouth. In such an event, the course of the disease is more serious.

Brazilian writers suspect that the vectors of the American disease are different species of small flies belonging to the genus *Phlebotomus*, in conformity with the widely accepted idea that similar Old World leishmaniases are transmitted by the same insects. The evidence advanced in support of this theory is most interesting, and all possibilities seem to point in this direction. Nonetheless, conclusive proofs are lacking which would permit calling *Phlebotomus* a vector of this disease with the same certainty with which we can connect *Anopheles* with malaria or *Aedes* with yellow fever.

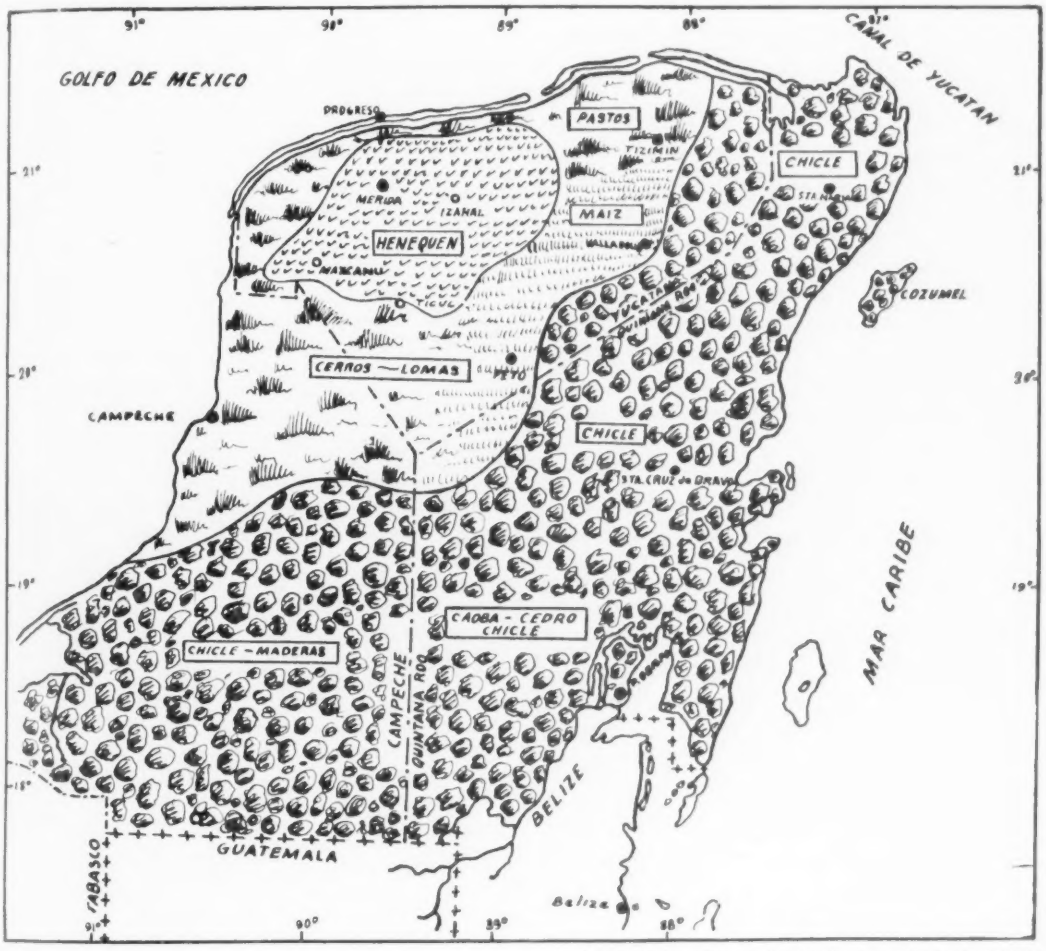
In Mexico, according to the little that we know about it, the disease begins and main-

En las zonas boscosas que cubren las dos terceras partes de la península de Yucatán, existe como una de las principales riquezas de la región el árbol llamado vulgarmente "chico zapote" (*Achras zapota*) cuyo latex, el chicle, sirve de materia prima para la elaboración de la goma de mascar, tan popular en los Estados Unidos y en nuestro país.

Restos mermados de las tribus mayas que antaño poblaron la península, y que construyeron las grandiosas ciudades de Uxmal, Chichen Itza, Tulum y tantas más, viven aun, en pequeños grupos en el centro de esas selvas. Pero la cantidad de habitantes permanente es bajísima en toda la zona.

La población de los bosques la constituyen, en su mayor parte, trabajadores que van contratados a la extracción del chicle o al corte de las maderas preciosas, seres aventureros que se internan en el infierno verde de la selva, en el que se pierden por varios meses, para robarle al bosque sus tesoros.

A fines del verano empiezan a llegar a la selva los chicleros, nativos algunos de la propia península y otros procedentes de diversas partes del país. Antaño arribaban en caravanas por senderos en el corazón de la jungla, que frecuentemente hay que mantener abiertos a fuerza de "machete"; hoy, en gran parte, son transportados en avión



Mapa de la Sria. de Agricultura y Fomento, México

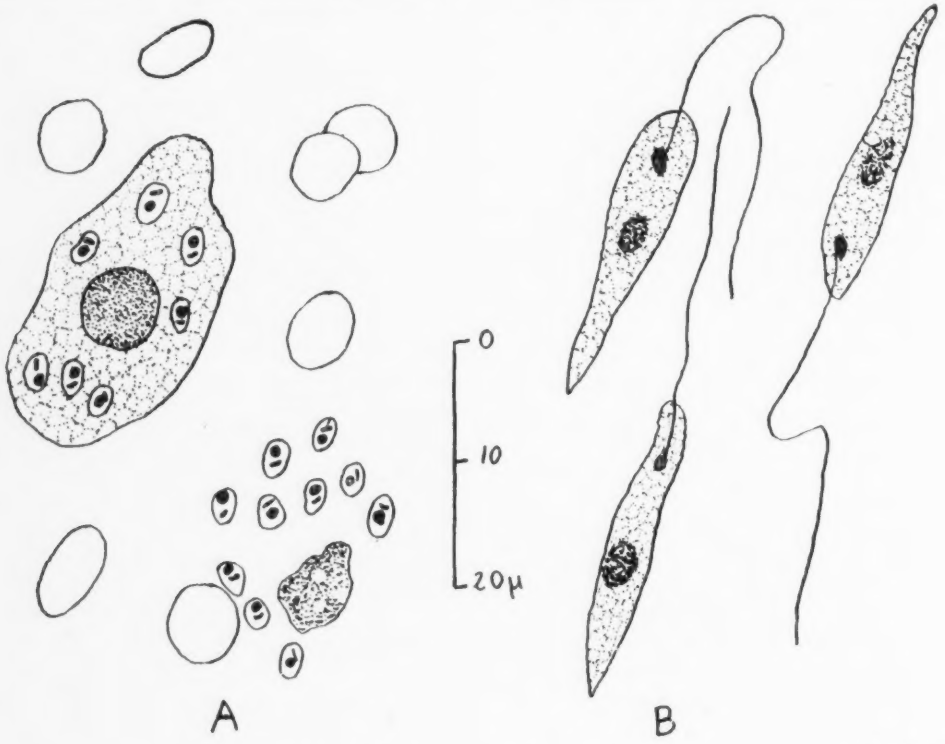
FORESTED AREAS OF YUCATAN WHERE CUTANEOUS LEISHMANIASIS OCCURS

ZONAS AGRÍCOLAS Y FORESTALES DE LA PENÍNSULA DE YUCATÁN. LA REGIÓN BOSCOSA ES EL ASIENTO DE LA LEISHMANIASIS CUTÁNEA.

tains a course similar to that observed in Brazil, but there are certain interesting differences. In the first place, the point of origin for the first lesions is the ears; all writers from Seidelin to the present day are agreed on this fact. In 1940, in the recently concluded epidemiological investigation, more than 80 per cent of the reported cases were localized in the earlobes. However, Professor Osorio Tafall this year visited some *chiclero* camps in Quintana Roo and brought back the interesting observation (personal communication, unpublished) that in the last two seasons the number of cases localized on the ears had diminished considerably, at least in the area visited, because the *chicleros* had begun to wear at their work a sort of leather helmet which covered their head, ears, and part of their neck, throat, and shoulders. Even those workers who did not take this care were acquiring the habit of wrapping a protective cloth around the head in the manner already indicated. Although the real value of such head protection remains to be determined statistically, the practice offers grounds for interesting speculations.

Chicle-workers' camps or *hatos* are hidden away in the very heart of the jungle. A small clearing bordered by sapodilla trees (*Achras zapota*), mahogany trees (*Swietenia mahagoni*), rubber trees (*Castilloa elastica*), and a thousand other plants serves as a site for several dwellings with palmleaf (*Inodes japa*) roofs. Supported by upright timbers and frequently without walls, these shelters naturally afford no protection against insects.

The few women and children of the group remain in these camps at all times. Very early in the morning, the men go forth to search out chicle trees that are ready for tapping. If a good quantity of chicle is desired without killing the trees, tapping cannot be done unless the plants have reached a certain age and have not been tapped for at least five years. On finding a suitable tree, the *chiclero* climbs to its top with the help of a



Dibujo Beltrán

LEISHMANIA BRASILIENSIS: A, FROM AN ULCER; B, FROM CULTURE MEDIUM
A. FORMAS EN UN FROTIS DE ULCERA. B. FORMAS FLAGELADAS EN CULTIVO EN MEDIO DE AGAR-SANGRE.

hasta las centrales y de ahí, a pie y en pequeños grupos, se internan a buscar los árboles de zapote que crecen silvestres en el bosque.

En nuestras investigaciones personales, iniciadas en el año de 1939 (*6*), hemos obtenido el dato que, de esos conjuntos de trabajadores, en su mayoría hombres, pero a los que también acompañan algunas mujeres y niños, aproximadamente el 5% regresa de la temporada afectado por la leishmaniasis forestal cutánea, la temida úlcera de los chicleros.

En Sur América, especialmente en el Brasil, donde se ha realizado el mayor número de trabajos al respecto, la enfermedad comienza en una forma casi imperceptible (*7*). El individuo nota en alguna de las partes descubiertas de su cuerpo (en aquel país predominantemente en los miembros inferiores) una pequeña zona enrojecida, semejante al piquete de alguno de los muchos insectos del bosque; la fuerte comezón que experimenta le obliga a rascarse repetidamente. Poco a poco se forma una pequeña pápula, luego un nodulito, que al poco tiempo se úlcera en su superficie, forma una costra y, sirviendo a la vez de asiento a otras infecciones asociadas, va avanzando realizando una labor de destrucción de los tejidos. Cuando las lesiones se encuentran en zonas como las piernas o los brazos, el pecho o la espalda, su evolución, aunque molesta y destructora, no suele ser de consecuencias. Cuando las úlceras están en la cara, el caso es distinto; no solo dejarán una cicatriz deformante sino que, además, en un crecido número de casos las mucosas de la nariz y de la boca serán también invadidas y, en tales ocasiones, la evolución del padecimiento es más seria.

Los autores brasileños (*8*) suponen que son pequeños moscos del género *Phlebotomus* (distintas especies), los que sirven de vectores a la enfermedad. Confirmando así la idea bastante aceptada de que las leishmaniasis del Viejo Continente son transmitidas por los mismos insectos. Las evidencias aducidas en apoyo de esa suposición son muy interesantes, y todas las posibilidades parecen apuntar en esa dirección. Sin embargo, faltan aun pruebas concluyentes que permitan acreditar el papel de transmisores a los *Phlebotomus*, con la misma seguridad que puede achacárseles a los *Anopheles* en el paludismo, o a los *Aedes* en la fiebre amarilla.

En México, de acuerdo con lo poco que sabemos al respecto, la enfermedad empieza y tiene una evolución semejante a la observada en Brasil, notándose sin embargo algunas interesantes discrepancias. En primer lugar, el sitio de elección para la aparición de las lesiones primarias lo constituyen las orejas; concordando en esto las observaciones de todos los autores, desde Seidelín para acá. En 1940, en la encuesta epidemiológica que llevamos a cabo, más del 80% de los casos reportados tenían su localización en el pabellón de la oreja. Sin embargo, en el presente año nuestro estimado amigo el Profesor Osorio Tafall, realizó una visita a algunos de los campos chicleros de Quintana Roo (datos no publicados), recogiendo la interesante observación, que bondadosamente nos ha comunicado de que, en estas dos últimas temporadas, el número de casos con localización en la oreja ha disminuído considerablemente, a lo menos en la región visitada por él, debido a que los chicleros han adoptado la costumbre de usar en su trabajo una especie de casco de cuero que les cubre la cabeza, las orejas, y parte de la nuca, los carrillos y el cuello; quienes no poseen esa prenda, están, sin embargo, adquiriendo el hábito de protegerse igualmente empleando alguna tela, con la que se envuelven la cabeza en la forma indicada. Este dato, que necesita una verificación estadística para encontrar su real valor, es muy interesante por las diversas sugestiones que de él parecen desprenderse.

Los campos o "hatos" chicleros están enclavados en el corazón mismo de la selva. Un pequeño claro, bordeado de zapotes (*Achras zapota*), de caobas (*Swietenia mahagoni*), de hule (*Castilloa elastica*) y de mil otras plantas, sirve de asiento a unos cuantos cobertizos con techo de palma de guano (*Inodes japa*), sostenidos por palos rectos, frecuentemente sin paredes y, naturalmente, sin elemento alguno de protección contra los insectos.

En dichos campos está permanentemente el pequeño número de mujeres y niños del grupo. En cuanto a los hombres, desde muy temprano, salen en busca de árboles de chicle

rope. Afterwards, he climbs down slowly, always aided by the rope which he holds with the left hand, meanwhile making oblique slashes with the machete carried in his right hand. These slashes are so placed that the greater part of them follow a definite herringbone pattern, each emptying into the opposite one below and leading to the foot of the trunk. Here there is placed a suitable container—generally a canvas bucket—which catches the sap that runs slowly from the wounds in the bark.

Each day's yield is carried to camp and dumped into great copper kettles that are placed on the fire to cook the chicle. In this manner are secured cakes of a definite size which are carried to the markets and for which the *chiclero* is paid by the pound.

Living in a damp and stifling climate characterized by heavy rains, exposed to the bites of a great variety of poisonous snakes, and eating an unbalanced and insufficient diet which must be supplemented with the meat of animals hunted in the forest, the *chiclero* stays in the jungle for three to six months at a time. No small percentage of the workers who return carry on their bodies the peculiar and tenacious ulcers caused by *Leishmania brasiliensis*.

At present, the injection of various antimony preparations (*Stibosan, Neostibosan, Fouadin*, etc.), which are highly effective in cases of leishmaniasis, has permitted a certain curative control of this disease. The *chiclero*, if he secures treatment in time, generally succeeds in halting the disease. With the rise of chemical therapy one sees less frequently those poor mutilated persons who lack part of an ear, an entire ear, and sometimes both ears, and who have the appearance of being victims of that medieval justice which used to punish certain crimes by amputating an ear.

Although there exist isolated and unconfirmed reports that suspected autochthonous cases of skin leishmaniasis have been found in other parts of Mexico, the only region where its presence has been established is in wooded parts of the Yucatán peninsula (the states of Yucatán and Campeche and the ter-

Foto Beltrán

A CHICLEROS' CAMP OR HATO IN QUINTANA ROO. SAPODILLA TREE IN CENTER

ASPECTO DE UN CAMPO O "HATO" CHICLERO EN QUINTANA ROO. EN EL CENTRO UN ÁRBOL DE CHICO-ZAPOTE.

propios para ser "picados," cosa que no puede hacerse sino en plantas de cierta edad y dejando transcurrir un término mínimo de cinco años entre cada "picada," si es que se quiere tener una buena cosecha de chicle y conservar la vida del árbol. Cuando encuentran uno adecuado trepan hasta su cúspide con la ayuda de una cuerda y después van bajando lentamente, siempre auxiliados con la cuerda que manejan con la mano izquierda, mientras con la derecha hacen cortes oblicuos con el "machete," de tal manera dirigidos que el superior vierte su contenido en el que le sigue y que tiene dirección opuesta, hasta llegar al pie del tronco donde colocan un recipiente adecuado, generalmente una bolsa de lona, para recibir el latex que va escurriendo lentamente por las heridas de la corteza.

La cosecha de cada día es llevada al campamento y reunida en grandes pailas de cobre, que se ponen al fuego para cocer el chicle y obtener de ese modo marquetas de determinado tamaño, que son las que se entregan en las centrales y cuyo pago se hace por peso al chiclero que las obtuvo.

En un clima húmedo y sofocante, con fuertes lluvias, expuesto a las mordeduras de gran variedad de serpientes venenosas, con una alimentación insuficiente y desbalanceada, que tiene que completar con los animales que caza en el bosque, el chiclero permanence en la selva por un término que varía de tres a seis meses, Y los que regresan, en no pequeño porcentaje, llevan en su cuerpo las peculiares y tenaces úlceras causadas por la *Leishmania brasiliensis*.

A la fecha, el uso de las diversas preparaciones inyectables a base de antimonio (Stibosán, Neostibosán, Fuadina, etc.), cuya acción en las distintas leishmaniasis es sumamente efectiva, han permitido un cierto control curativo sobre esta enfermedad. El chiclero, si se trata a tiempo, generalmente logra que se detenga el padecimiento, y cada día es menos frecuente ver esos pobres mutilados a quienes falta parte de una oreja, la oreja, entera, y a veces ambas orejas, lo que les da la apariencia de ser víctimas de aquella justicia medioeval que castigaba ciertos delitos con la amputación del pabellón auditivo.

Aunque existen reportes aislados y no comprobados, de supuestos casos autóctonos de leishmaniasis cutánea encontrados en diversos sitios del país, la única zona donde ha sido establecida la presencia de este padecimiento es en las partes boscosas de la península de Yucatán (Estados de Yucatán y Campeche y Territorio de Quintana Roo) y en

Foto Beltrán

THE JUNGLE, QUINTANA ROO

ASPECTO DEL BOSQUE EN LA REGIÓN DE BACALAR, QUINTANA ROO. AL FONDO EL DR. ROBERT HEGNER, FRENTE A UN ÁRBOL DE CHICO-ZAPOTE (*Achras zapota*).

las partes limítrofes de los Estados de Chiapas y Tabasco (*9*). Sin embargo, muy recientemente, por cortesía de los Drs. Martínez Báez y González (datos no publicados), tuvimos oportunidad de ver un caso de leishmaniasis cutánea en un niño (cuyo diagnóstico se comprobó microscópicamente), nativo de Huajuapan de León, Oaxaca, y que, según los datos disponibles, nunca había abandonado su residencia. Este hallazgo, sobre cuya significación no podríamos pronunciarnos antes de hacer un estudio que tenemos proyectado, es por demás interesante no sólo

ritory of Quintana Roo) and in neighboring areas of the states of Chiapas and Tabasco. Very recently, however, through the courtesy of Drs. Martínez Báez and González (data unpublished), we had the opportunity of seeing a microscopically confirmed case of skin leishmaniasis in a boy, a native of Huajuapan de León in Oaxaca who, according to information available, had never been away from home. This discovery, the significance of which I cannot judge until I have carried out a projected study, is interesting not only because the case was found more than 600 kilometers from the zone of classic leishmaniasis, but also because the characteristics of the region, at 1,597 meters altitude and with a dry climate, are absolutely different both physiographically and biologically from those of the southeastern jungles.

Given the geographical limitation of the disease, its relatively benign form in Mexico (it is much less grave here than in Brazil), and the existence of drugs of sufficient effectiveness to treat it, it cannot be said that forest leishmaniasis of the skin amounts to a first-rank public health problem in Mexico. However, it has a number of aspects which give it great scientific interest.

In Mexico lies the northern limit of the continental distribution of the disease. Consequently, it follows that it is highly important to establish the exact limits of its distribution there. It is equally important to define more closely the characteristics of the infection and of its etiological agent, comparing them with the findings in the republics to the south. It can also be stated that a series of clinical differences exist which would justify this study.

Its method of transmission in Mexico is not yet known. The inhabitants of the affected region generally assign the role of vector to the so-called *mosca chiclera* or "chiclero fly" (*Olfersia coriacea*), which attacks various gallinaceous birds in that region, and even Bequaert has been led to mention this as possible. However, our own

Foto Beltrán

THE AUTHOR (LEFT) AND DR. HEWITT STANDING BEFORE A CHICLERO'S HUT

CHOZA DE CHICLEROS, TECHADA CON PALMA DE GUANO (*Inodes japa*). AL FRENTE, EL DR. REDGINAL HEWITT (CON CASCO) Y EL AUTOR.

porque el sitio está a más de 600 kilómetros de los límites de la zona leishmaniásica clásica, sino porque las condiciones de la región, a 1,597 metros de altura y con un clima seco, son absolutamente diferentes de las de los bosques del sureste, tanto fisiográfica como biológicamente.

Dada la limitación geográfica del padecimiento, la forma relativamente benigna que toma entre nosotros (mucho menos grave que en el Brasil) y la existencia de productos terapeúticos de bastante eficiencia para su tratamiento, no puede decirse que la leishmaniasis forestal cutánea constituya en México un problema sanitario de primera importancia. Sin embargo, existen una serie de aspectos que le dan gran interés científico.

México constituye el límite septentrional de la distribución continental de la enfermedad y, en consecuencia, resulta de la mayor importancia establecer los límites exactos de su área en nuestro país. Es igualmente importante precisar mejor las características del padecimiento y de su agente etiológico, comparándolas con las bastante conocidas que presenta en las Repúblicas del Sur. Desde luego, puede decirse que existen una serie de diferencias clínicas que justifican este estudio.

Su mecanismo de transmisión no es aun conocido en México. Los habitantes de la región atribuyen generalmente el papel de vector a la llamada "mosca chiclera" (*Olfersia coriacea*) que parasita en diversos gallináceos de la región, y aun Bequaert (*10*) ha llegado a mencionarla como posible; sin embargo nuestras observaciones realizadas en la zona chiclera nos inclinan a no considerarla con tal carácter. Aunque no está probado, puede considerarse como probable que los *Phlebotomus* sean sus transmisores, tal como se supone en Brasil, donde aun se han señalado las especies que parecen más sospechosas.

Su aparente preferencia por el sexo masculino adulto (los casos en niños y en mujeres son en menor porcentaje) constituye otro asunto de gran interés para ser investigado, tanto desde el punto de vista epidemiológico como experimental, tratando de aclarar si, como generalmente se supone, ello se debe únicamente a la mayor exposición por parte de los hombres, o existe algún otro factor que interviene en el asunto.

El estudio de los problemas inmunológicos, tanto de reacciones diagnósticas intradérmicas u otras, como de la aplicación de vacunas, iniciado con éxito en Brasil, presenta

Foto Beltrán

CHICLERO AND CHICLE PAN

TIPO DE "CHICLERO" DE LA ZONA DE QUINTANA ROO. JUNTO A ÉL LA PAILA DE COBRE EN QUE CUECEN EL CHICLE.

entre nosotros un campo virgen en el que pueden obtenerse muchos resultados, tanto científicos como prácticos.

La relativa facilidad con que se obtienen y mantienen cultivos vigorosos de estos protozoarios en medios de agar y sangre, ofrece un amplio campo de experimentación muy interesante, y que todavía tiene una serie de puntos por investigar.

Por otra parte, los datos que hasta la fecha existen con respecto a la susceptibilidad de diversos animales capaces de ser empleados

observations in the *chiclero* zone lead us to discount this view. Although it is not proved, it can be considered probable that *Phlebotomus* is the carrier, as supposed in Brazil, where the most suspicious species have already been signaled out.

The apparently greater incidence of the disease in adult males (only a small percentage of cases occur in women or children) constitutes another aspect of great investigative interest from the epidemiological, as well as experimental, viewpoint. It would be interesting to ascertain if, as generally supposed, this is due solely to the greater exposure on the part of the men, or whether some other factor enters into it.

Immunological problems, like diagnostic skin tests and the use of vaccines, already begun with success in Brazil, present in Mexico a virgin field in which many scientific and practical results may be obtained.

The relative ease with which these protozoa may be started and maintained as vigorous cultures in media of agar and blood offers an ample field for very interesting experimentation on a number of points which still require investigation.

Unfortunately, the present data on the susceptibility of the various animals that may be used with confidence in laboratory projects are contradictory. This is true even of the Syrian hamster (*Cricetus auratus*), which has not proved as good an experimental host to leishmaniasis of the skin as it was to visceral leishmaniasis. On the other hand, various squirrels and related species (*Citellus, Sciurus*, etc.) that are native to North America appear to offer possibilities in this field.

Therefore, although forest leishmaniasis of the skin cannot rank in Mexico with malaria, amoebic dysentery, onchocerciasis, and other parasitical diseases in public health importance, it is of enormous interest from the scientific point of view.

The emergency conditions now existing throughout America have shown us in an inescapable manner the stern necessity of understanding better all our native diseases, including their etiological, epidemiological, clinical, pathological, therapeutic, and immunological aspects. This need is even greater with respect to those diseases which, hidden in the jungle, conceal their real importance until the necessities of the moment, such as troop movements, road building, airfield construction, or the utilization of new zones of production, make us realize that even today there is much we do not know about vast territories in this immense hemisphere that now fights for its liberty and its future.

con confianza en los trabajos de laboratorio son contradictorios. Y esto es cierto aun en el caso del "hamster" de Siria (*Cricetus auratus*) que en la leishmaniasis cutánea no ha resultado ser tan buen huésped experimental como en el de la leishmaniasis visceral. En cambio, diversas ardillas y sus parientes (*Citellus, Sciurus,* etc.) nativos de nuestro Continente, parecen presentar posibilidades en este sentido.

Así pues, si por su importancia sanitaria la leishmaniasis forestal cutánea en México no puede compararse con el paludismo, la amibiasis, la onchocerciasis y otros padecimientos parasitarios, desde el punto de vista científico presenta un enorme interés.

Las actuales condiciones de emergencia en que vivimos en todo el Continente nos han marcado, en forma ineludible, la necesidad absoluta que tenemos de conocer mejor todas aquellas enfermedades que nos son propias, enfocándolas desde los puntos de vista epidemiológico, etiológico, clínico, patológico, terapeútico, inmunológico, etc. Y esta necesidad es quizá mayor en el caso de aquellos padecimientos que, escondidos en la selva, suelen parar inadvertidos en su real importancia, hasta que necesidades del momento como transporte de tropas, construcción de caminos o campos de aterrizaje, utilización de nuevas zonas de producción, etc., nos hacen comprender que hay todavía mucho que ignoramos en las vastas tierras de este inmenso Hemisferio, que hoy lucha por su libertad y su futuro.

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THE EARLIEST WINGED FISH-CATCHERS

By E. W. GUDGER

SINCE birds are the best known winged fish-catchers, the title may conjure up a vision of small flocks of gulls of our northern harbors busily at work getting their dinners; or it may recall the far larger flocks of fish-catchers (terns, cormorants, and pelicans) which populate the keys of southern Florida. But, to see such winged fish-catchers in really surpassing numbers, one must go with Robert Cushman Murphy to his *Bird Islands of Peru*. If one cannot see these wondrous bird haunts in person, then one can at least envisage them in his most interesting and informative book just named (New York, 1925).

Around these Peruvian islands, which though inside the tropics are cooled by the north-flowing Humboldt Current, are found small fishes, especially anchovies, in such astronomical numbers as to defy computation. Feeding on these are marine birds in clouds darkening the sky from which they "descend in rainstorms" to merge on the surface in great rafts, covering acres and acres of ocean—areas so large that they are visible miles away. These millions of birds with insatiable appetites devour billions of fishes each day. Murphy estimates that "Thousands of tons of these fishes per day are devoured by sea birds which breed on the Peruvian islands." And earlier than he, R. E. Coker judged that a single flock of cormorants observed at the Cincha Islands would consume each year a weight of these fishes equal to one-fourth the entire annual catch of the fisheries of the United States.

These feathered fishermen are, however, the latest forms of all winged fish-catchers; to describe the earliest is the purpose of this article. These feather-winged ones are the highest developed in structure, activities, and numbers of all winged fish-feeders. In each of these points they are far removed from those creatures, buoyed on leathern wings, that sought their ichthyological dinners in the bays and seas of Mesozoic or middle geological time. In fact these earliest winged fishers were not birds at all, nor even the ancestors of birds, but in time and structure were far removed from them. They were the winged dragons of the Jurassic period. And after them came the toothed birds of the Cretaceous.

THE PTEROSAURS OR FLYING LIZARDS OF THE JURASSIC PERIOD

The pterosaurs (ter-o-sawrs) were, as their name indicates, winged lizards. They are also sometimes called ornithosaurs (or-ni-tho-sawrs) or bird-lizards, because they could soar like birds; but they were reptiles adapted for flight. Like some birds they had long beaks but their jaws were toothed. These pterosaur teeth were reptilian, being sharp, conical, and implanted in the jaws. Moreover, they were in all respects organs for grasping, like the long, pointed, rather slender, backwardly hooked teeth of the fish-eating crocodiles of the Ganges. Being carnivorous, these flying dragons had to eat either fishes or other reptiles, and their teeth indicate a fish diet.

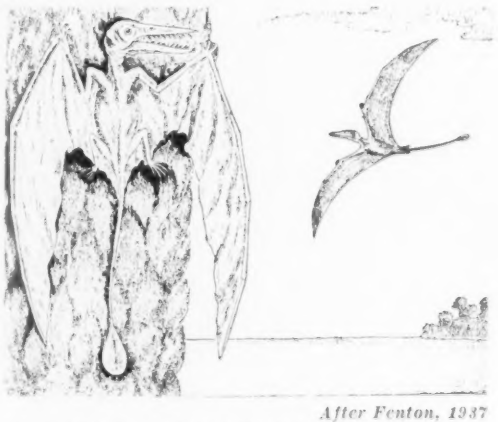
They flew somewhat like birds, and their general birdlike appearance was accentuated by the fact that the head was set on top of the spinal column almost at a right angle to the neck. Their forelimbs were adapted for a birdlike flight, the outer or fifth finger being enormously elongated into a boom or yard, the other fingers being free. From front leg and "little finger" there extended back along, and attached to, the side of the body the membranous wing, which included the hind limb, except the foot. The opposite wings joined on the tail (Fig. 1).

The pterosaurs were the most highly specialized of all backboned animals. As in birds, all the bones of the body were hollow and were lighter in structure than in any animals present or past, birds not excepted. In some forms the walls of the bones were as thin as a desk blotter. These had their extremities strengthened internally by delicate paper-thin struts of bone. All these light bones, like those of our present-day birds,

had openings that permitted the circulation of air. This structure gave these flying dragons an almost unbelievable lightness of body, as we shall see later. The surface area of their wings was, when compared with the weight of their bodies, greater than that of any other flying animals past or present, save only some of the insects.

Because of their enormously elongated outer fingers and the wings which they supported, these flying lizards got their common name, "wing-fingers." These pterosaurs varied widely in size—from that of a sparrow to giants of eight feet in length with a wing-spread of twenty feet. With bodies lightened by their thin-walled bones, they careered on leathern wings above the bogs, swamps, lakes, and seas of the Mesozoic era of the earth's far distant past, seeking what they might devour (Fig. 1).

However, the best authorities agree that the power of flight in these dragons of the air was much more limited than in birds. There is great doubt as to the ability of the pterosaurs to fly by flapping their wings. The structure of their skeletons did not offer points of attachment for the great muscles necessary for prolonged flight by beating the air. Perhaps, however, they could flap their wings enough to help get a start for soaring flight. The paleontologists think that the pterosaurs were gliders and soarers, taking advantage of ascending air currents as do our gliders today. But then comes the question of the take-off. If they alighted on trees



After Fenton, 1937

FIG. 1. RHAMPHORHYNCHUS
ONE CLINGS TO BARK OF A TREE WITH CLAWED FEET; ANOTHER SOARS OVER A LAKE ON WIDESPREAD WINGS.

From a drawing in American Museum

FIG. 2. RHAMPHORHYNCHUS AT REST
FROM ITS PERCH IT COULD TAKE OFF, GLIDE, SOAR. AS ILLUSTRATED, THE BEAK IS TOO SHORT OR THICK.

or cliffs or any sharp elevation, they might launch forth fairly easily. But, if they should light on level ground or on the surface of the sea, how could they ever get up again? They were poorly adapted for walking, perhaps better for climbing by means of their clawed feet. So betwixt climbing and scrambling, they may have ascended trees and rocky declivities to where a take-off was possible, after the fashion of any gliding bird of today (Figs. 1 and 2). However, fly they did, since, even more than birds, they were fitted only for an aerial life.

These strange flying lizards, the most highly specialized and extraordinary animals that have ever lived on our earth, appeared suddenly in the Upper Triassic of Europe about 150 millions of years ago. Widely scattered over the earth, they flourished for millions of years and as suddenly as they appeared so suddenly they disappeared toward the close of the Cretaceous period, about 60 million years ago. In consonance with their extraordinary physical make-up are the equally extraordinary facts that they

From Zittel-Eastman, 1932

FIG. 3. SKULL OF CROOKED BEAK

THE HEAD IS AT A RIGHT ANGLE TO THE SPINE AND THE IRREGULAR TEETH POINT OUTWARD AND FORWARD.

came without known ancestors and became extinct without leaving descendants.

There were numerous genera and species of these flying dragons. The earlier ones had long lizard-like tails; the later forms had vestigial tails and were probably the better fliers. Certainly among them were the largest flying animals the world has ever seen. For food possibly (indeed probably) all of them, among other edible things, caught and ate fishes as is indicated by the structure of their teeth. Our attention will be concentrated on the three well-known forms —Rhamphorhynchus, Pterodactylus, and Pteranodon—all surely fish-catchers.

Rhamphorhynchus (ram-fo-rin′-cus) or Crooked Beak, so called probably because of his strangely set teeth (Figs. 1, 2, and 3), was a relatively small flying lizard (about 1½ feet long) notable for having long, narrow, pointed wings like those of a gull or a tern, and a long slender tail ending in a spatulate organ which probably functioned as a rudder (Figs. 1 and 4). The position of this rudder, whether horizontal or vertical, has been much discussed. This unique structure was long held to have had a vertical position like the tail or rudder of an airplane, but a leading authority has concluded, from a critical study of the best material, that it was horizontal as is the tail in all other flying animals, save the so-called flying fishes.

Equally characteristic were the long pointed jaws filled with large, acuminate, irregularly (crookedly?) set teeth pointing outward and forward instead of backward as one would expect in a fish-catcher (Fig. 3). There were multitudes of fishes in the Upper Jurassic seas, and these Rhamphorhynchus must have taken on the wing. But it is somewhat difficult to understand how in full flight he could catch and hold his slippery, scaly prey with these forwardly pointing teeth. Then when he had caught a fish, how could he, even by elevating his head, have got it started down his throat against the opposition of these wrong-way-pointing teeth? The paleontologists give no answer to this question. He presumably ate fishes, but

From Barnum Brown by courtesy of Sinclair Refining Company

FIG. 4. RHAMPHORHYNCHUS IN FULL FLIGHT—SEEN FROM BELOW

THE LONG, NARROW WINGS ARE BOOMED OUT BY THE LIMBS AND THE PRODIGIOUSLY ELONGATED FIFTH FINGER OF EACH HAND. THE SPATULA AT THE END OF THE TAIL PROBABLY SERVED AS A HORIZONTAL RUDDER.

surely no fish-eater ever had teeth so ill adapted for catching or swallowing fish.

Furthermore, when this strange little beast came to make a landing, he probably did not light on the ground because his feet were weak and small. Since his long tail would be an encumbrance on the ground, he probably lighted on a tree trunk to which he could cling with the clawed toes of all four feet (Fig. 1). Up its rough bark this small pterosaur could climb until he reached a branch (Fig. 2) from which he could launch forth in flight. In Figure 1 the flying wing-finger is portrayed in oblique dorsal view, and in Figure 4 he is again portrayed in full flight, but from the ventral aspect.

The drawings showing the structures and activities of this and other flying dragons are of course restorations based on the bony framework of the beasts, but clothed by the imagination of the artist, who was guided, however, by the critical eye and judgment of the scientific man. The artist of Figure 4 was supervised by Barnum Brown, to whom I am indebted for this and other illustrations and for many essential facts about the three wing-fingers studied herein. For further information the reader should consult Brown's "Flying Reptiles" in *Natural History*, Vol. 52, pp. 104–111, 1943.

Pterodactylus (ter-o-dak-till-us), literally "wing-finger," was a very short-tailed, but long-necked, flying dragon. It was larger

After Barnum Brown, 1943

FIG. 5. PTERODACTYLUS ALIGHTING
SMALL, CONICAL, UPRIGHT TEETH ARE CONCENTRATED IN THE FRONT PART OF EACH JAW OF THIS WING-FINGER.

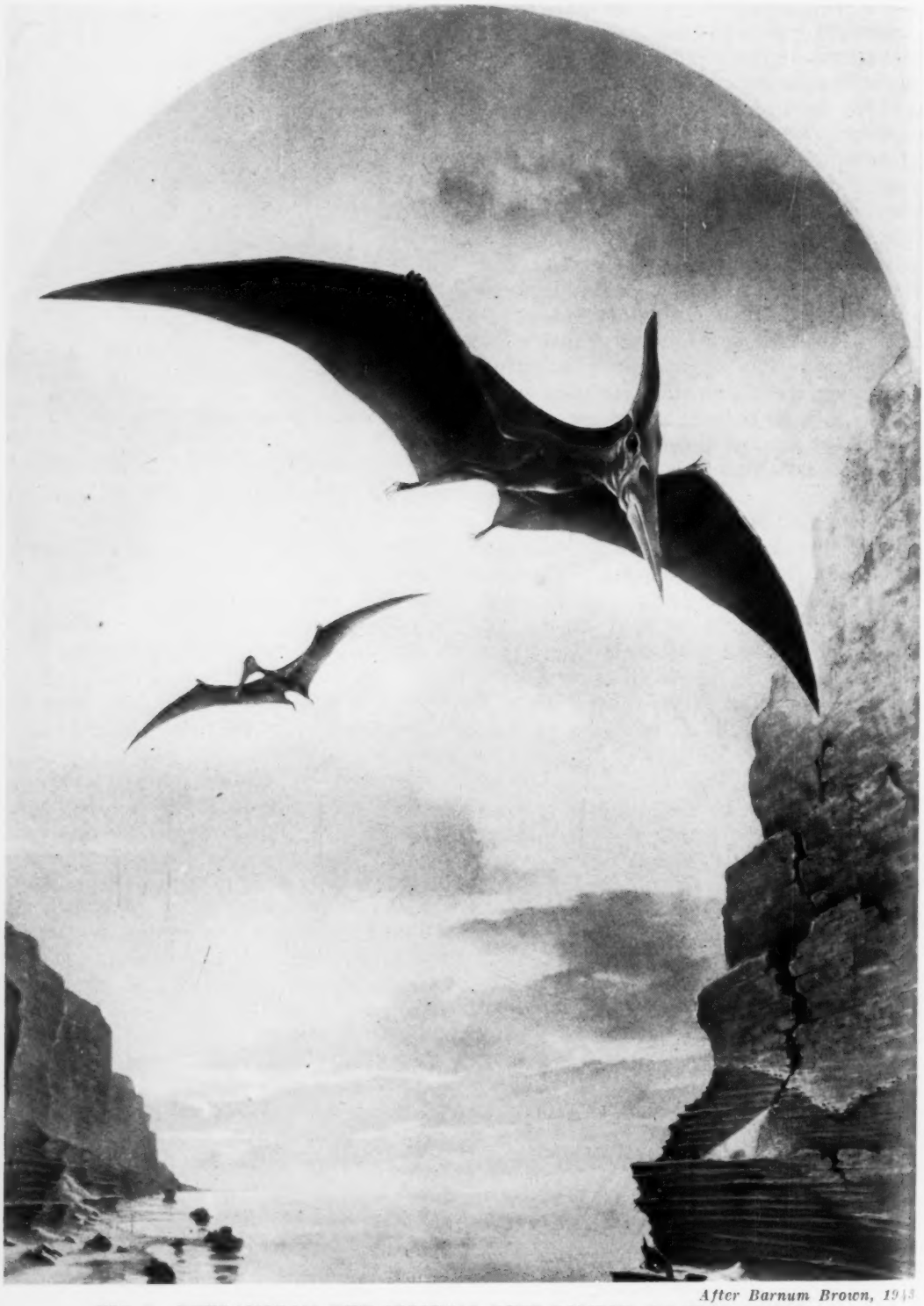
After Abel, 1919

FIG. 6. PTERODACTYLUS FEEDING
SWOOPING DOWN ON WIDESPREAD WINGS, THIS FLYING DRAGON PICKED HIS FISH FROM THE WATER SURFACE.

and, as restored by the artist under supervision of Barnum Brown, more grotesque—in short far more weird in appearance (Fig. 5) than Rhamphorhynchus. Some species attained the size of an eagle and had longer limbs than the Crooked Beak. The wings of Pterodactylus were larger and heavier, and his small teeth, concentrated near the tip of each jaw, were conical and stood upright. They were much better adapted for catching fishes than those of the flying lizard just described.

Pterodactylus probably caught and ate more fishes than his distant cousin, Rhamphorhynchus. Of this activity, there is fortunately a splendid portrayal made under the supervision of one of the greatest authorities on the pterosaurs, Prof. Othenio Abel of the University of Vienna (Fig. 6). Swooping down on wide-extended wings, Pterodactylus picked up his surface-swimming prey as our present marine birds do.

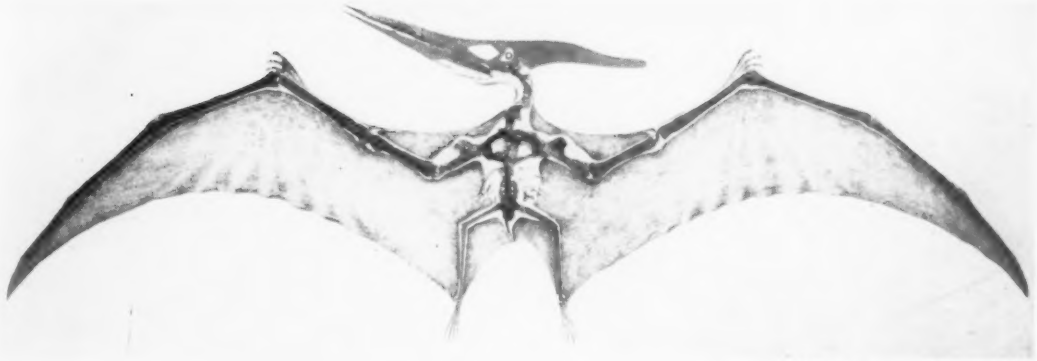
Pteranodon (the toothless winged one) was the largest and, because of his extraordinary wing development, the best flyer of all the pterosaurs. His head was drawn out in front into long, dagger-shaped, toothless jaws and behind into a long thin supra-occipital crest. Brown thinks that the latter probably served as a rudder when this flying dragon dived headlong at its prey (Fig. 7). If it so functioned, it took the place of the spatulate rudder on the tail of Rhamphorhynchus.



After Barnum Brown, 1943

FIG. 7. PTERANODON, THE ORIGINAL DIVE BOMBER, IN FULL FLIGHT.

IT WAS THE LARGEST FLYING ANIMAL OF ALL TIME, ACCORDING TO PALEONTOLOGISTS. FROM THE FRANK BOYDEN SWEET MURAL ("WINDOW TO THE PAST") IN THE AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK.



After Barnum Brown, 1943

FIG. 8. THE SKELETON OF PTERANODON SHOWN IN VENTRAL ASPECT

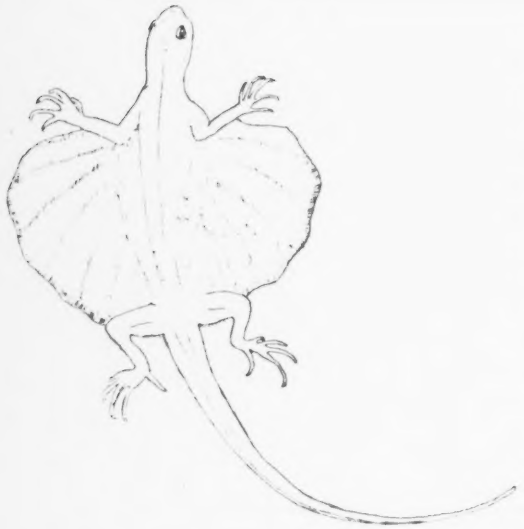
THE SKULL IS PRODUCED IN FRONT INTO LONG TOOTHLESS JAWS AND BEHIND INTO A LONG BONY CREST. THE SKELETON IS FUSED TO FORM A STRONG SUPPORT FOR THE WINGS. IN AN 8-FOOT-LONG SPECIMEN THE WINGSPREAD WAS 20 FEET, BUT THE HOLLOW BONES CAUSED IT TO WEIGH ONLY ABOUT 25 POUNDS.

The most perfect skeletons have been found in the Niobrara Cretaceous deposits of Kansas. The anterior limbs, and especially the "little fingers," are enormously elongated, and the wingspread of some 8-foot specimens was 20 or more feet. Despite the great wingspread, these light-boned animals weighed surprisingly little, probably not more than 25 pounds for a 20-foot specimen. Compare it with an albatross having an 11-foot spread and a weight of about 17 pounds. Brown thus characterizes the strongly knit skeleton: "A rigid framework of fused backbones, shoulder girdle, and breastbone furnished support for the enormous wings and for the attachment of muscles necessary to hold them in position, an arrangement unparalleled for strength among other animals." This highly coordinated bony framework is admirably portrayed in Figure 8, in which, however, the breast bone is not shown, since it has been removed so that the fused wing-bearing bones may be seen.

The paleontologists have long been puzzled over the food of the toothless Pteranodon, but they conjectured that it must have been fishes taken on the wing. This conjecture has recently been proven correct by the preparation here in the American Museum of the lower jaw of a specimen. In the pelican-like throat-pouch between the bones of the lower jaw were found the vertebrae of two kinds of fishes—the remains of the last supper of this particular flying dragon. For taking fishes his toothless jaws were no more ineffective than are those of a tern or gull. What an awesome sight it would have been, could one have seen a flock of these Pteranodons—the largest animals that have ever flown, the original dive bombers—swoop down on twenty-foot wings (Fig. 7) over a school of fishes in the Cretaceous seas of the Kansas of the Mesozoic era. Since they were extremely highly specialized, the Pteranodons became extinct as the Mesozoic closed.

Before leaving the flying dragons, it will be interesting to quote Sir Richard Owen: ". . . the flying reptile with outstretched pinions must have appeared like the soaring Roc of Arabian romance, but with the features of leathern wings with crooked claws superinduced, and gaping mouth with threatening teeth."

The extinct pterosaurs were the only reptiles that have ever really flown. However, among present-day animals of this group are some that are called flyers. The best example is the so-called "flying dragon," *Draco volans*, of the East Indies and southeastern Asia (Fig. 9). It has an umbrella-like membrane on each side from front to hind limb, each being partially free. These membranes are distended partly by the limbs but mainly by the movable ribs. They act simply as the wings of a glider and enable this lizard to glide or volplane from one limb of a tree to another, to another tree, or to the ground. But Draco cannot soar as did the pterosaurs nor flap its wings as the flying lizards may have done. Among mammals,



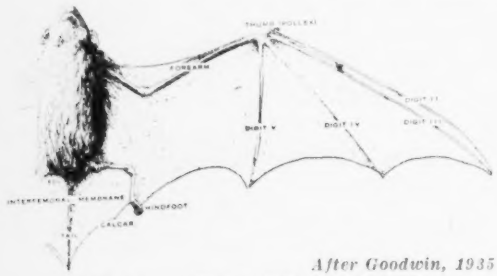
From H. G. Seeley, 1901

FIG. 9. THE FLYING DRAGON

THE "WINGS" OF *Draco volans*, FOR GLIDING ONLY, ARE SUPPORTED BY LIMBS AND BY MOVABLE RIBS.

the well-known flying squirrel has a similar membrane, unsupported by ribs, with which it also glides.

The thoughtful reader has already contrasted the pterosaurs with the only mammals that have ever really attained the power of flight—the Bats or Chiroptera (hand-winged ones). Every one knows that the bat has on the side a membrane extending from the front limb backward to, and embracing, the hind limb and at the short tail coalescing with the membrane from the other side (Fig. 10). But the pterosaur had all the digits of the forelimb free save the last (the little finger), which, acting as a boom, stretched the wing membrane taut for flight; the bat has only the thumb free, while the second and third fingers are tied fast to the wing. The four fingers act like the ribs of an umbrella to distend the membrane. To aid in this distension is the hind limb with the elongated great toe fast to the edge of the hinder part of the wing, the other toes being free. The tail also plays its part in this distension (Fig. 10). In the pterosaur the hind limb did its share of stretching the wing, while even in the tailed forms the tail played only a little part. With these shorter but stronger wings bats can both beat the air and soar, whereas the pterosaur was practically confined to soaring.



After Goodwin, 1935

FIG. 10. A BAT AND ITS WING

THE THUMB ONLY IS FREE; THE FINGERS BOOM OUT THE WING, ACTING LIKE THE RIBS OF AN UMBRELLA.

But the chiropterans are not direct descendants and close relatives of the pterosaurs. Bats are mammals and are far removed in time and structure from the reptiles. They have fragile bodies and wings, and these and their manner of life make their fossilization a matter of great difficulty. Their fossil remains are very rare. Consequently it is not known when they first appeared, but quite surely not in the Mesozoic. Their earliest remains have been found in the Eocene, the opening period of the Age of Mammals. But it should be noted that these fossils were "perfectly good" bats.

Whether these early winged mammals were fishers like the pterosaurs cannot be said, but it is probable that they were not. However, as I have shown in two articles, there are scattered around the world today some bats that are fish-catchers and fish-eaters. Thus *Pizonyx* in the Gulf of California is a fish-eater. So also in India and Burma certain false vampire bats (genus *Megaderma*) eat fishes. But the best known fish-catching bat is *Noctilio leporinus*, the rabbit-nosed bat of the Caribbean region. From time to time numbers of these bats have been seen fishing around Monos Island off the northwest coast of Trindad. Specimens taken at Monos and in Surinam have been found to have their stomachs crammed with fish remains. An article on this fish-catcher is in press.

THE ODONTORNITHES OR TOOTHED BIRDS OF THE MESOZOIC ERA

Of all fossil remains, those of birds are among the rarest, and accordingly they are among the most highly prized. And this is

After Lucas, 1901

FIG. 11. *HESPERORNIS REGALIS*, THE GREAT WESTERN DIVER, IN ACTION
THIS HUGE BIRD, SIX FEET LONG, WAS THE GREATEST SWIMMER AND DIVER THE WORLD OF BIRDS HAS EVER HAD.

particularly true of the Odontornithes, the toothed birds. All of us have used the phrase "as scarce as hens' teeth" to emphasize the fact that the thing referred to simply does not exist. Yet in the upper Jurassic and middle Cretaceous deposits are found remains of birds with toothed jaws. Two of these birds flew with bird-feathered wings and the third was a wingless diver.

The earliest of these was Archaeopteryx, ancient winged one, or ancient flyer of the Upper Jurassic. It was feathered and flew with feathered wings but these feathers only thinly hid its reptilian characters; it was about equally lizard and bird. It was a forest dweller, and since it did not catch fishes, it may be passed by with these few brief words. The other Odontornithes were true aquatic birds and fish-catchers, which came on the scene thousands of years after Archaeopteryx, the lizard bird.

For all their equipment of teeth, these Odontornithes were true birds. Externally they had the form of birds and were covered with feathers; rudimentary perhaps but still feathers. One, though flightless, had some hollow bones and the rudiments of wings; the other, with a highly pneumaticized skeleton and well-developed wings, was an excellent flyer. In their fossilized skeletons are some reptilian characters, but their bony framework is overwhelmingly bird; they were birds even if they did have teeth.

They were discovered by the eminent paleontologist, O. C. Marsh of Yale University, in 1870–1872, in the Middle Cretaceous beds of western Kansas and were described by him in a publication entitled *Odontornithes: Monograph on the Extinct Toothed Birds of North America*, Washington, 201 pp. 1880. In those middle geologic days, what is now the Great Plains region was a shallow sea which extended from the Rocky Mountains quite 500 miles to the east, with unknown northern and southern limits. In this sea were a great variety and abundance of fishes, and here the toothed feathered fish-eaters likewise flourished.

The two forms to be studied were birds, but were the descendants of rapacious rep-

After Knipe, 1905

FIG. 12. *ICHTHYORNIS VICTOR*
THE LAST OF THE TOOTHED BIRDS, THIS FISH-BIRD, ABOUT THE SIZE OF A PIGEON, WAS A STRONG FLYER.

tilian flesh-eaters. They possessed not a few reptilian characters but the most obvious was their sharp-pointed, conical, backwardly hooked teeth composed of dentine covered with smooth, shining enamel—teeth eminently reptilian. Now our present-day birds have bony jawbones covered with horny sheaths. These sheaths are sometimes jagged or serrate but never toothed. The Odontornithes lacked these horny sheaths and had their teeth implanted in the jawbones. The two toothed birds being studied were contemporaries, but very different in certain structures and in manner of life. However, they had in common long spearlike jaws beset with recurved teeth—almost perfect instruments for the capture, retention, and ingestion of fishes.

Hesperornis regalis is the first to be considered. In 1871 O. C. Marsh was the fortunate discoverer, in the Niobrara deposits of western Kansas, of an almost complete skeleton of the "Royal Western Bird." And such indeed it was, for it measured nearly six feet from tip of beak to end of toes. However, it was flightless, having a flat (nonkeeled) breast bone and only the remnant of the upper wing bone (humerus) covered over by the skin and possibly by muscles. Even lacking wings it was still completely aquatic, since its powerful legs and marvellously developed swimming feet admirably fitted it for such a life (Fig. 11). Like the present-day loon it probably spent little time ashore save for nest-building and breeding.

Also in keeping with its flightless aquatic life was its complement of teeth, the lower jaw being toothed throughout and the hinder two-fifths of the upper jaw likewise so armed. These teeth had large bases and were set in a continuous groove in each jaw. These jaws with their conical, sharp-pointed, and backwardly hooked teeth were admirably adapted for catching and holding active, scale-clad, slippery fishes. The teeth were plainly reptilian, but on the other hand, from a fossilized impression of the skin of the upper leg, Hesperornis is thought to have been completely covered with smooth, soft, hairlike feathers (Fig. 11) similar to those found on the present-day flightless, but land-dwelling, Apteryx of New Zealand.

Everything about Hesperornis attests that it was the largest and most powerful swimming and diving bird that has ever lived. Lacking even rudimentary wings, but with the long-beaked head set at the top of a long flexible neck, its jaws beset with sharp hooked teeth, and its long and powerful legs ending in huge swimming feet, it was a fish-catcher of the first rank among diving birds past or present (Fig. 11).

Ichthyornis victor, the other toothed bird, was also discovered by Marsh and in the same Cretaceous deposits in Kansas. Only one restoration of the bird as it looked in the flesh has ever been made, so far as I know. Seen in Figure 12, it shows a small, well-built, pigeon-like bird. It had a rather long neck ending in a large head and a long bill beset with strong teeth. In marked contrast to the mouth and forwardly pointing teeth of the flying lizard, Rhamphorhynchus, the jaws and backwardly hooked teeth of the toothed bird, Ichthyornis, surely constituted a far more efficient fish trap. The legs and feet are small in comparison with those of Hesperornis, but also in marked contrast are its long powerful wings. So much for its general external appearance.

But to see how well this flying fisherman

was equipped to take fishes at the surface of the water, the skeletal structure must be studied. No complete skeleton has ever been found, but Marsh has portrayed a restoration with the missing bones in outline (Fig. 13). From this it is seen how weak was the leg development, and in contrast how powerful were the wings. Then note the relatively huge, plowshare-like sternum for the attachment of the great flying muscles. Long, strong wing bones, big keeled sternum, and skeleton highly pneumaticized throughout, all proclaim Ichthyornis to have been a strong flyer—in marked contrast to the subaquatic manner of life of the Great Diver.

These avian characters are very apparent and tend to obscure the less apparent reptilian ones; nevertheless, these are present in many parts of the skeleton. The arrangement of the teeth in both jaws duplicated that of Hesperornis. These reptilian teeth were smaller, were sharp-pointed and strongly hooked toward the gullet. Unlike the teeth of Hesperornis, each tooth was set in a distinct socket. Another skeletal character in which Ichthyornis especially differed from the Great Diver and from all other birds is found in the vertebrae. Marsh (p. 119) states that "... their biconcave vertebrae separate them widely from all birds recent and extinct, and point back unmistakably to a very lowly ancestry, even below the reptiles." And later he more explicitly says (p. 179) of these biconcave or fishlike vertebrae: "This form is seen in a few recent and in many extinct Reptiles, and in the Amphibians; but it is especially characteristic of Fishes, from which it was undoubtedly inherited by the superior groups. This character alone indicates great antiquity for the class of birds"—and especially for that one fittingly named Ichthyornis, (*ichthys*, fish; *ornis*, bird).

Ichthyornis was distinctly an aquatic bird, seeking its piscine prey in the warm Mesozoic seas. On this point Marsh gives evidence that "Its food was probably fishes, as their remains are found in great abundance mingled with those of Ichthyornis." Since its bones are found in the same Cretaceous deposits with those of the toothless Pteranodon, Ichthyornis must have competed with the latter for the fishes in the waters above which they both flew.

After Marsh, 1880

FIG. 13. SKELETON OF ICHTHYORNIS

NOTABLE ARE THE WEAK LEGS, THE POWERFUL WINGS, THE HUGE KEELED STERNUM, AND THE TOOTHED JAWS.

With the passing of the Mesozoic and the advent of the Cenozoic, or era of present-day life, the flying dragons and the toothed birds pass from the scene. Their successors were the ancestors of the birds of our times. And so we return to the gulls, terns, cormorants, and pelicans of our shores and of the waters around the *Bird Islands of Peru*.

SCIENCE WORKS WITH THE ARMED FORCES*

By REAR ADMIRAL J. A. FURER

WHEN Dr. Jewett invited me to make a few remarks at this meeting, he said that I might talk on any subject that would help the scientist to see the point of view of the military man, and vice versa. So I am going to discuss the human relationships between scientists on the one hand and professional Army and Navy officers on the other. In my job as the Navy's Coordinator of Research and Development, I find that I spend more time coordinating human beings than I do in coordinating research and development. And I must confess that there seem to be times when the tables are turned and other human beings begin to coordinate me.

Early in my present assignment I discovered that the scientist and the military man have a common denominator. The common denominator is intellectual honesty. I have been impressed also with the fact that the world of science is not so impersonal and far removed from the common touch as is often supposed. I have found that scientists and professional military men alike are motivated by the same basic instincts and ideals—by pride in accomplishment, by desire for recognition for a job well done, by a passion for service, and by a deep sense of responsibility. And—a fact too seldom appreciated—I found that scientists are generally intensely practical people. Once they are given the military considerations involved, they can drive forward to a common-sense solution of a problem with a directness that comes as a surprise to those who think of scientists as living on a plane beyond the everyday world.

You will note that I have emphasized *professional* military men. The vast majority of officers in time of war are not professional Army and Navy officers. The reserve officers, for example, who deal with scientific matters are mostly scientists themselves, and they are a fine lot. It is the professional officer—the officer who, because of his training and experience *must* fill the higher position of responsibility—who needs most to see eye to eye with the scientist. In other words, it is the "Brass Hat" versus the "Great Brain" bogey which pops up to plague us from time to time.

It is perhaps only natural that "brass hattism" easily becomes a term of opprobrium in the public mind whenever anything goes wrong. Gold braid on the cap visor becomes the mark of a closed mind and a skull that requires trepanning to inject a simple idea. As a matter of fact, a lifetime of experience and the wartime responsibilities that go with brass hats usually make the wearer a pretty sound individual. I know of very few cases in which scientists and brass hats have not acquired considerable respect for each other's abilities—often to the astonishment of both parties!

In the mobilization of science for war, I am particularly proud of our mutual achievements in working out the human problem. It is not always easy to resolve the different points of view of the scientist and the military officer. In many respects our backgrounds are very dissimilar. In normal times the scientist looks upon the world as a friendly world. He likes to take all mankind into his confidence as soon as he makes a discovery. He wants his inventions and discoveries put to prompt use. He has a natural human desire to receive credit from his profession and from the public for the fruits of his labors. By contrast, the professional Army and Navy officer must think of the world as a potentially hostile world. It is his job to plan for the defense of his country, even in times of apparent peace. He cannot take the world into his confidence because one of the fundamental precepts of military science is to withhold from the enemy as long as possible all information about plans, weapons, and resources.

One of the unheralded victories of this war is the satisfactory manner in which these divergent points of view have been resolved. Today, the war has fused the scientific and the military mind in the common mold of

* An address before a closed meeting of the National Academy of Sciences in Washington, D. C., April 24, 1944.

public service. The scientist has been wise and patient in not clamoring for immediate credit for his contribution to the war effort. Publication of certain scientific papers has been cheerfully suspended for the duration. In the laboratory and in the field the scientist has worked tirelessly and without recognition, realizing that such recognition must, of necessity, be deferred until the cessation of hostilities. He has deferred to the necessities of what is commonly called "red tape," and he has done this despite the fact that his whole individualistic existence cries out against the formalized, channelized procedure which the Army and Navy tell him is necessary in dealing with so complicated a problem as modern warfare.

In bringing together the scientist and the military man, we of the military believe that we, too, have contributed materially to the good will and understanding that have been established. We, too, have made concessions for the common cause. There are few regular Army and Navy officers who pretend to be scientists, just as there are few scientists who pretend to be experts in applying scientific thinking to the making of war. We of the Navy have made every effort to remember our own shortcomings and to enlist the aid of the scientist in making up our deficiencies. It can also be fairly said that we have made every effort to eliminate red tape for your convenience. In the Navy I have constantly striven to get our officers to explain to the scientist why we do things as we do them, to overlook the petty irritations that arise in joint undertakings, and to forget personalities. Our prime responsibility is to win the war, and our goal is to enlist in this effort every ounce of assistance that we can summon from the civilian world of science.

Many of your fraternity have been taken into our full confidence on the highest levels. Dr. Bush, as Chairman of the Joint New Weapons Committee, is at an unprecedentedly high military level for a scientist. The views of eminent scientists in the National Defense Research Committee are continually sought and are respected. A great many scientists have traveled extensively in the theaters of war. Through the recently established Office of Field Service, scientists are beginning to appear wherever our forces are engaged.

If I were to take a poll of this audience to determine your chief problems in dealing with the military, I believe the answer would be "red tape" and "security," in that order. Now "red tape" is something that I have no desire to defend if the phrase is employed in the uncomplimentary sense in which it is generally used. But I will undertake to defend it if defined in the sense that I would define it—namely, as *orderly procedure*. We all like to dispense with procedure and to arrive at immediate decisions. We like to cut out the "middleman" and to shorten the chain of command to get things done. In other words, we like to take short cuts. There are times when short cuts are good and there are other times when they are not. If you are late catching the bus in the morning and take a short cut across your neighbor's lawn, he will not object if it happens once or twice. But if one hundred other people began taking short cuts across his lawn, that would be different. I suspect he would set up signs and try to channelize the traffic around his lawn. Now in organizations as large as the Army and Navy, it is absolutely necessary to set up procedures to direct the traffic and to fix authority and responsibility. These procedures are not needed perhaps in your laboratories, or, for example, in a small business establishment. But the Navy is no longer small business. Orderly procedure is necessary to avert traffic jams. And the need for orderly procedure increases arithmetically, if not geometrically, as the size of the undertaking increases.

You may think my remarks are purely academic. But let me cite an incident where failure to follow orderly procedure came near to causing trouble. A few months ago, I arranged for a small group of scientists and an officer from my staff to visit a unit of the fleet to talk over some problems. The Admiral, his staff, and the scientists quickly found a number of problems that merited research attention. At this point the next step should have been a letter from the Admiral to the cognizant Bureau, putting the wheels in motion. But instead, negotiations were started directly between the Admiral and the scientists to work out these problems without reference to the Bureaus concerned. I believe that both the Admiral and the scien-

tists were quite unaware of the importance of getting the ultimately responsible bureau people into the picture from the beginning. As soon as the chairman of the appropriate NDRC committee and I became aware of the situation, we got the matter straightened out before it had done any more damage than to upset a few of the most competent officers I know in the Navy. It was not merely a matter of feelings. The people in the Bureaus were ultimately responsible for the work and thus were placed in the untenable position of having to accept responsibility without being apprised of the undertaking, and, what is more important, the scientists might have gone along on the work without benefit of the experience of the Bureaus.

Do not get the idea that I or most Naval officers make a fetish of following the Mosaic law. I myself have frequently lifted projects out of the routine so as to shorten the chain of command in order to get a job started more quickly. This cannot be done in all cases and, if attempted, must always be done with judgment and discrimination; otherwise there is no gain.

Therefore, if I do say a kind word for "red tape," I do so only out of the depth of my long experience. Someone has to look out for the best interests of the whole fleet, and that is all that the "Brass Hats" are trying to do in stressing orderly procedure.

As for the other most audible complaint—the handling of security, we of the Army and Navy appreciate the cooperation you are giving us in trying to keep our plans and devices away from the enemy as long as possible. Scientists are playing the game of security with commendable forebearance and sportsmanship. But before you complain about the stringency of some of the rules, let me hasten to confess that I myself do not think that the rules are always sensibly applied. I am constantly battling to get the rules changed where necessary. While we all agree that we should prevent helpful information from reaching the enemy, as I see it this policy is sometimes carried to the point where we keep much needed information away from our own people. I think it is particularly shortsighted to restrict too closely the number of scientists who are permitted to know about important research projects. My platform is that anyone who can contribute in wartime to the more *speedy* solution of any of our problems should be brought into the picture. On the other hand, it goes without saying that there is no justification for disclosing classified information merely to satisfy someone's curiosity, no matter how exalted that person's position may be. Of course, no matter how discreet those may be who are in on a secret, the probability of a leak increases geometrically with the number of people who know about it. The number is, however, at the very most not large during the research and development stage as compared with the number who must know as soon as the device is placed in service.

Almost any reasonably sound proposal for a new device can be realized if enough talent and enough money are provided. There is no such thing as permanent secrecy of ideas, or even very much lag in the flowering of the same ideas in the brains of the enemy. I plow through vast heaps of intelligence reports and am pretty well convinced that the enemy is trying to do all—or most all—of the things we are, and is certainly thinking along virtually the same lines. True security lies in speed of accomplishment. It is the only way we can keep ahead of the enemy in this complex technical war of measure and countermeasure. Together we examine the problem, you furnish us the scientific alternative solutions, we both weigh the corollary problems of manpower, cost, time, and production; and, once the decision is made, our real security lies in how soon the fruits of the job will bring grief to our enemies.

The following lines from Kipling's ballad *The "Mary Gloster"* often strike me as particularly appropriate to the technological warfare being waged between American scientists and the enemy, especially the Japs:

> They copied all they could follow,
> But they couldn't copy my mind,
> And I left 'em sweating and stealing
> A year and a half behind.

It is that jump on the enemy that counts these days, and only with the unselfish devotion of scientists in laboratories all over the country can we maintain and widen our lead.

ARE WE FACING SEVEN LEAN YEARS?

By CARL F. TAEUSCH

DURING the past seven years, agricultural production in this country has been nothing short of bounteous. From the standpoint of physical production, these have been fat years; certainly with respect to such major crops as corn, cotton, and wheat, and probably also for other crops and livestock products. What is the likelihood for the future?

"Man lives not by bread alone," said Robert Louis Stevenson; "he also lives by catch phrases." And the biblical story of Joseph has been ringing in our ears long enough to provide a fertile field for the warning that, now that the seven fat years have passed, the next seven years may not be so plenteous. The warning is supported by the reminder that the weather, one of the most potent single factors in farm production, has been unusually favorable in the recent past, and that we may soon face the adverse trends of the rainfall cycle. In a sense, this warning may also reflect man's pessimistic proclivity to say, "This good cannot keep on indefinitely." Hearing these ominous notes, what can we conclude as regards the future?

The hazards of prophecy are at their maximum when agricultural production is being forecast. This is the case, not only because of the indeterminable weather factor, but also because of the bases on which the forecasts are so often predicated. Like the trite inquiry, "Will the sun rise tomorrow?," too many affirmative answers are based on the proposition, "Because it always has." And the fact that the two major fallacies in this answer were pointed out long ago, does not seem to disturb this kind of prophet. We do not, as a matter of strictly observed fact, know that it is true that "the sun has always risen"—some people never have seen the sun rise!—and even if it were true, that does not help us with respect to tomorrow's sun. The fact that crop statistics have been available in this country only since the War between the States and are not too reliable in the earlier part of the record limits us in any attempts to be even as general in our observations as we are in regard to past appearances of the sun. And whether next year's crops will follow the patterns of the past is not only questioned by such cyclical views as that of the seven fat and seven lean years, but the prophecy also often stumbles against the hard fact of an extremely exceptional year or the beginning of a reversal of past trends.

What can we say, therefore, even in general, as to the prospects for the immediate years ahead? Is there a method which is better than that of the observer on a hill who has seen the sun rise a number of mornings in succession and who may have his prophecy confirmed tomorrow? Can we approach the matter more as the astronomer might, without even observing the rising of the sun, by discovering the various factors at work in a complex system that is not subject to daily or annual hazards?

We do need to look at the past record. And, although we shall confine ourselves to the production trends of three of our major crops—corn, cotton, and wheat—we can here point out certain other factors in the agricultural production situation. Agricultural production in recent years has been characterized by a shift from the earlier preponderance of field crops as the major source of farm income to the present preponderance of livestock and livestock products as the major direct source. Therefore, the relatively declining importance of field crops as a direct source of gross farm income would make recent increases in production and in absolute income value all the more significant; and they are as feed the indirect source of much of the absolute and relative increases in farm income from livestock production. Furthermore, even among the field crops, these three staple commodities have recently been supplemented by other farm products, sometimes at their expense. Thus, the Agricultural Adjustment program encouraged the diversion of crop acreage to increased pasture land, and the outbreak of

the war and the cutting off of our southwestern Pacific sources of vegetable oils witnessed a tremendous diversion of staple-crop acreage to the production of soybeans, peanuts, and similar crops. The resulting decline in corn, cotton, and wheat acreage makes all the more striking the phenomenon of the past seven years, in which the production of these staple crops was not only maintained but even increased.

Thus, in spite of a reduction of acreage in corn during the past seven years, amounting to some 9 million acres, or 9 per cent from the previous 38-year average, the total annual number of bushels of corn actually produced each year from 1937 to 1943 has been among the greatest in our history, averaging almost 7 per cent greater than the previous 38-year average. Cotton acreage, during the past seven years, declined relatively even more than did corn acreage: over 9 million acres, a decline of almost 27 per cent from the previous 34-year average. And yet total annual cotton production has recently averaged slightly higher than it ever did prior to 1937. Wheat does not present such a clear-cut picture. The past seven years have shown an 18 per cent increase in average total annual production over the previous 40-year average, but total acreage has averaged 8 per cent higher than before 1937.

These production records are cited without referring to the need or market demands for the three commodities. However much "the law of supply and demand" may operate elsewhere, demand does not operate so simply in determining agricultural production. Not only is there a considerable lag in its effectiveness; but often the lack of demand expressed in a low price, has stimulated the farmer in some years prior to 1937 to increase his production in order to maintain his gross income. The last seven years, it must furthermore be remembered, did not include the two drought years, 1934 and 1936; but the figures for the preceding 34 to 40 years with which comparisons were made, included enough years to reduce appreciably the effects of these drought years on the average annual production figures. We may consider ourselves extremely fortunate that we had such abundant production recently; markets have been stimulated directly and indirectly by the war, and we are even using up the cotton and wheat surpluses that for a while seemed so menacing. But we are disassociating here any relationship between demand and production, and are centering our attention on production data only. Market and demand considerations, and weather conditions, however they may operate as causes, should not blur the brute fact that agricultural production, at least of corn, cotton, and wheat, has been characterized by abundance during the past seven years.

We ask, "Will this abundance continue in the immediate future, or are we facing a reversal of trend so that seven lean years are in the offing?" A frank answer would honestly have to take account of the possible vagaries of market conditions as well as of the weather; and it would have to be, "We don't know." We should not, however, allow any answers to go unchallenged which rely merely on folklore or on catch-phrases or loose analogies. And we may be able to formulate a probable answer on the basis of factors which underlie the whole production situation, factors of which the production figures are symptomatic. And some of those factors may be discovered by analyzing past records of acreage, production, and yields per acre to see what has determined our agricultural production.

Agricultural production in America has been determined largely by the number of acres in cultivation. It may seem to some to be obvious that corn production, for example, is a product of the yield per acre by the number of acres in corn, which it is; and that total production varies equally with, or proportionally to, both acreage and yield. But, strange as it may sound, this latter statement is not the case. In general, ever since the War between the States, changing production has been more nearly proportional to changes in acreage than to changes in yields. As the West was settled, and new corn land was opened up, the total national production of corn increased, and it increased pretty much in proportion to the increase in acreage. And this relationship between acreage and production was very

close, in view of the fact that national-average yields per acre remained almost constant, when averaged over 15 or more years, until fairly recently. It has been only comparatively recently, as the acreages of corn, cotton, and wheat have become stabilized or have declined, that one can see—as one would expect—an appreciable and increasingly close correlation between yields per acre and total production.

This phenomenon of practically constant yields, prior to 1937, is startling. It would evidence no appreciable effect of all the educational work of the colleges of agriculture and the federal and state departments of agriculture, at least in the direction of improved farm management which presumably would result in increasing yields per acre. The pre-1937 record of yields also stands out sharply in contrast with that of the past seven years, during which time this previous record of constant yields, over periods of that many or more successive years, has been broken for the first time since the War between the States. From 1867 to 1936 corn production averaged over successive periods of 15 or more years and for the total 70-year period almost exactly 26 bushels per acre. The more recent part of that 70-year record, namely 1899–36, averaged even slightly less than the earlier (1867–98) part of that period, showing that there was no pre-1937 trend toward increasing yields of corn per acre. For the past seven years, however, the national average yield of corn per acre has been slightly over 30 bushels. So also with cotton. Yields averaged around 180 pounds per acre from 1876 to 1936. From 1876 to 1902 they were slightly more; from 1903 to 1936 they were slightly less. During the past seven years, cotton yields have averaged almost 250 pounds per acre. Wheat averaged from 13 to 14 bushels per acre from 1866 to 1936, with little variation in the averages of successive periods of 13 to 18 years during that time. For the past seven years, wheat has averaged over 15 bushels per acre, with the more recent years showing even higher figures.

How can we account for this interesting phenomenon of constant yields of these three major commodities over so long a period prior to 1937? Acreage and total production were both increasing appreciably, but they were increasing proportionally to each other. True, as crop acreage was increasing, it was extending itself into the newer western areas and the result may have been that the richer and more productive or the drier and probably less productive areas neutralized any possible decreases or increases in crop yields in the older, eastern areas. But a study of yields in the older agricultural states shows the same phenomenon of relatively uniform yields throughout the total pre-1937 period. Weather conditions, especially favorable rainfall or drought, may have increased or decreased production and yields in particular years. But in general, the correlation between acreage and production, prior to 1937, was much higher (0.89 for corn, 0.93 for cotton, and 0.95 for wheat) than it was between yields and production (0.473 for corn, 0.407 for cotton, and 0.647 for wheat). However we may account for the phenomenon, the fact seems to be clear that, at least prior to 1937, the total national production of corn, cotton, and wheat, was much more nearly proportional to acreage than it was to yields per acre. During the past seven years, beginning with 1937, the reverse has been true: annual production totals of corn and cotton have been much more nearly proportional to yields per acre than to number of acres harvested, and the total annual production of wheat has been relatively more so.

Now, it is conceivable and highly probable that, with vast areas of cheap land available, farmers preferred to engage in what may be called extensive farming, rather than in the more expensive operations of intensive cultivation which might have produced higher yields. Higher yields, as such, are not necessarily synonymous with sound farm management. Land was cheaper than labor, and it could be expected that cultivated acreage would be extended westward rapidly as an expression of more intelligent management than one which would adopt the policy of more intensive operation in the older eastern areas. Furthermore, it is understandable why the Agricultural Adjustment program of the early 1930's adopted the methods it did: crop-production changes from year to

year had for a long time been closely proportional to annual changes in acreage; therefore any program of crop curtailment could be expected to be achieved best by a reduction in acreage. What actually happened—that suggested acreage curtailments were not fully effected, that two severe droughts curtailed production through declining yields as much as the program had contemplated effecting through reduced acreage, and that the more intensive cultivation of the reduced acreage may have neutralized in part the objective of the program—does not affect the main proposition, that yields had been a relatively constant factor in American agricultural production up to 1937, and that changes in total crop production, at least until 1937, had been pretty closely proportional to changes in the amount of acreage harvested.

What has happened during the past seven years, therefore, stands out all the more clearly by contrast with the preceding record. Increased or maintained total national production of corn, cotton, and wheat is not a new story in American agriculture. But it is something new that production has been increased over a period of several successive years without increasing acreage, or that it has been maintained with decreasing acreage. What has happened has been the startling increase in yields.

Corn, with a relatively constant national average yield of less than 26 bushels per acre for some 70 years prior to 1937, or for periods of 15 or more years during that time, has averaged over 30 bushels per acre during the past 7 years, and immediately following a 38-year average of less than 26 bushels per acre. In the last 7 successive years, the minimum average national yield has been 28 bushels per acre; in 1942 the yield was just under 35 bushels per acre. In 1943, the yield was 30 bushels per acre, a figure achieved by any annual yield only four times in the previous 70 years.

Cotton, with average annual yields previously ranging from 160 to 190 pounds per acre for periods of 13 years or more, and a national pre-1937 average of 180 pounds, has averaged 248 pounds per acre since 1936. This latter figure had never before been achieved in any single year, the highest previous yield having been 225 pounds; in only 12 years previously did cotton yields exceed 200 pounds per acre. In 1942, the yield was 275 pounds per acre; in 1943, 259 pounds.

Wheat yields during the latter part of the pre-1937 period showed a slight rise over the earlier part of that period—from around 13 bushels per acre to around 14. The past seven years have averaged 15.3 bushels per acre. In 1942, the yield was 18.65 bushels per acre; in 1943, it was 16 bushels.

It is generally agreed that favorable weather had much to do with these unusually high yields during the past seven years. Rainfall was abundant and generally came at the right time in the growing season. When it did not—last year too much rainfall in the spring retarded planting and rotted much seed in the ground—later warm weather overcame this handicap. Rainfall has been increasing since the drought years; we are still in the upswing of the rainfall cycle, and this fact provides the pessimists among the prophets with their strongest argument that lean years are looming ahead.

But other factors are also at work, and they are significant as regards future possibilities. Selection and use of better seed has worked for higher yields, especially since 1936 as regards corn and cotton. The extension of the use of hybrid corn has had nothing short of phenomenal results. Iowa has averaged over 50 bushels of corn per acre during the past seven years, a yield of 60 bushels having been reached in 1942. Illinois, Ohio, and Indiana averaged over 45 bushels per acre since 1936. Average national yields in the future are bound to reflect the extension of hybrid corn into the western and southern areas, where yields have been less than 20 bushels per acre and where hybrid corn has only tardily been introduced. Cotton yields in the bigger areas of Texas and Oklahoma rose above 175 pounds per acre during the past seven years, a large yield for that region. In the Mississippi Delta region, yields averaged well above 325 pounds per acre, Alabama and Georgia topped 230 pounds, and the Carolinas were well over 300 pounds to the acre.

These are unusually high yields for these respective areas, and they have been pretty well sustained during the past seven successive years. True, a high-yielding seed may not necessarily produce the better grades of cotton—we are actually facing an insufficient supply of the longer staples—but whatever the high total-production figures may signify, economically or otherwise, they have been increasingly the result of higher yields, and a part of this higher yield can be attributed to better seed. Uniform-staple communities have been increasing, and the securing of better grades of seed—aside from their high-yield virtues—is one of the important functions of such organizations. Where the higher-yielding seeds are adopted by such communities because they also provide uniform staples that are in relatively greater demand, the resulting production is of especial value. The wheat situation is not so clear, and the more modest increases in recent national average yields may be due solely to the weather. At least there is less evidence of the prevalent use of the higher-yielding varieties of seedwheat, although there has always been a demand for, and increasing use of, disease- and drought-resisting varieties, which contribute to increasing yields.

Better cultivation, increased mechanization, greater use of fertilizers, and other improved farm practices, including the use of adequate amounts of insecticide on cotton, have also contributed to the results of higher yields. The balancing of land values against labor costs may determine the relative swings to more or less intensive cultivation, but the completion of the settlement of our best lands has more recently weighted the scales heavily in the direction of better utilizing available land, especially by increasing yields. The soil-conservation practices begun in the early 1930's at that time salvaged our fast-wasting soil; now those soil-building practices are beginning to pay dividends by also contributing to higher yields. The "ever-normal granary" is not confined to stocks of food or fiber on hand; it also consists in providing a reservoir of well-conditioned soils. The diversion, to other crops and to pasture, of low-yielding corn, cotton, and wheat land has reduced considerably the acreage devoted to the three staple crops; while this may seem to achieve a result of higher yields by statistical methods,[1] it is indicative of better farm management. And the maintenance of, or even increase in, the total national production of corn, cotton, and wheat on the reduced acreage, makes the resulting total national production as well as the higher yields per acre all the more significant, in view of the fact that the additional crops and pasture produced on most of those diverted acres practically constitute a net increase in the nation's food supply.

The greater productivity of the past seven fat years thus comes to be seen as a result of many factors, which do not by any means resolve themselves merely to the vagaries of the weather. It is highly doubtful that there will be any cyclical swings in better seed selection or in other forms of better farm management that were learned and applied during the past decade. They are here to stay. Labor shortages and difficulties in securing farm machinery, fertilizer, and insecticides may temporarily check any proclivities toward further intensive cultivation, but these will not apply to the postwar period. Price and farm income trends may seriously interfere with the continued operation of the controllable production instruments which have recently resulted in higher yields, but they are also in part man-made and are therefore amenable to intelligent legislative policies and individual cost controls. In short, it seems as if we face the possibility of intelligently continuing the operation of many of the factors which have effected the phenomenally high yields of corn, cotton, and wheat during the past seven years, and therefore of providing an abundance of food and fiber for a war-ridden world. The weather may qualify the results,

[1] Abandoned acreage (acres planted but not harvested) may at first appear as even a more extreme case of the same potential fallacy. Although a part of the abandonment of planted acreage results from drought, floods, hail, etc., some of it also arises from the fact that the original planting of such acreages was due to poor judgment. In any case, the factor does not enter into our calculations; the figures here cited are based on harvested acreage.

but not to the extent of facing us with an inevitability of seven lean years.

Our dependence in past years on a vast and untouched domain for the greater part of our expanding production, has had to give way before the brute fact that that source ceased to exist. Recently, our reliance has shifted to methods of more intensive cultivation, in which the abundance of our farm crops has been obtained by increasing yields. Underlying this situation appears to be the fact that farm production, which formerly increased largely through the opening up of new land areas and which was largely subject to the vicissitudes of the weather, more recently has resulted from controllable factors involved in intelligent farm management. If increased or maintained total farm production is needed in the future to feed our own people adequately as well as for export, we need to increase either net acreage or yields to accomplish the purpose. Increased acreage on any considerable scale is out of the question; good farm management may even require the diversion of existing poor cropland to other purposes. Thus we are practically forced to rely on increased or maintained yields for producing the farm products which are to supply future needs. Inasmuch as increased production in the past was obtained largely through increased acreage, because land was cheaper than labor, we may now be faced with higher farm-labor costs, if we are to increase or maintain yields. But this problem can be met by increasing the efficiency of farm labor, a possibility which has been resorted to by industry and commerce, but which is merely on the threshold of being utilized in agriculture. From the standpoint of physical production, therefore ,aside from economic events and policies that may disturb the situation, there is every reason to believe that the past seven-year record of abundance can in large part be continued in the future by increasingly intelligent farming.

APPENDIX

Tables 1, 2, and 3 were compiled so as to show trends and interrelationships in acreage, production, and yields of corn, cotton, and wheat.

The periods were selected, first, by grouping five sets of approximately equal numbers of consecutive years, differing for each commodity, and beginning with the earliest available records. These periods were then checked for the relative homogeneity of their constituent years. This was done by shifting the terminal years of each period to the adjacent period; if the resulting arrangement increased the homogeneity of both periods, the process was repeated until the maximum of homogeneity of the annual figures within each of the five periods was achieved for each crop. The annual averages were then calculated for each period; and the percentages of these averages, with reference to the over-all annual average, were calculated.

This method brings out clearly the relative constancy of the average yields for the successive periods of time for each commodity, except for the more recent years. It also presents, when the percentages of acreage and production are compared, a rough basis for showing the close relationship of these two factors, again until the higher yields of the more recent years changed the situation.

The method of multiple correlations provides some substantiating evidence verifying these results, especially as regards the relationships over the longer periods of time. It does not, however, supplant the method of averages here employed, especially for the successive shorter time periods, because it does not disclose the differing bases—of average acreage, production, and yield for the respective periods—upon which rest the correlations shown in the following table.

VALUE OF r (CORRELATION COEFFICIENT)

Periods	Acreage: Production			Production: Yield		
	Corn	Cotton	Wheat	Corn	Cotton	Wheat
Over-all (68–78 yrs.)	0.877	0.871	0.942	0.530	0.424	0.667
Prior to 1937 (61–71 yrs.)	0.891	0.928	0.949	0.473	0.407	0.647
1937–43	0.519	0.980	0.274	0.888	0.785	0.546
Earlier part, pre-1937	0.922	0.936	0.937	0.463	0.715	0.689
Later part, pre-1937	0.423	0.746	0.796	0.956	0.689	0.605

TABLE 1. CORN (ANNUAL AVERAGES)

Period	No. of years	Acreage (millions)		Production (million bushels)		Yield (bushels per acre)	
1867–1943	77	84.039		2,205.79		26.195	
		Mill. acres	% of 1867–1943 aver.	Mill. bu.	% of 1867–1943 aver.	Bu. per acre	% of 1867–1943 aver.
1867–83	17	51.0	60.69	1,308	59.28	25.65	97.93
1884–98	15	79.4	94.48	2,079	94.24	26.14	99.79
1899–1915	17	97.4	115.95	2,628	119.15	26.98	103.01
1916–32	17	102.0	121.40	2,646	119.97	25.94	99.01
1933–43	11	92.9	110.58	2,434	110.33	26.29	100.37
1867–98	32	64.3	76.52	1,669	75.67	25.88	98.80
1899–1936	38	99.4	118.31	2,561	116.10	25.74	98.26
1937–43	7	90.7	107.94	2,731	123.82	30.10	114.91

TABLE 2. COTTON (ANNUAL AVERAGES)

Period	No. of years	Acreage (millions)		Production (million bales)		Yield (pounds per acre)	
1876–1943	68	27.31		10.6718		187.05	
		Mill. acres	% of 1876–1943 aver.	Mill. bales	% of 1876–1943 aver.	Lbs. per acre	% of 1876–1943 aver.
1876–88	13	16.0	58.63	5.965	55.90	171.6	91.74
1889–1902	14	22.9	83.75	9.052	84.83	189.0	101.05
1903–15	13	31.5	115.24	12.613	118.16	190.9	102.04
1916–29	14	36.8	134.91	12.672	118.74	163.4	87.37
1930–43	14	28.8	105.61	12.862	120.52	217.8	117.35
1876–1902	27	19.6	71.65	7.566	70.90	180.6	96.57
1903–36	34	34.0	124.44	12.679	118.81	172.8	92.39
1937–43	7	24.7	90.61	12.902	120.89	248.3	134.59

TABLE 3. WHEAT (ANNUAL AVERAGES)

Period	No. of years	Acreage (millions)		Production (million bushels)		Yield (bushels per acre)	
1866–1943	78	45.0		623.855		13.683	
		Mill. acres	% of 1866–1937 aver.	Mill. bu.	% of 1866–1937 aver.	Bu. per acre	% of 1866–1937 aver.
1866–78	13	23.8	52.78	296.9	47.59	12.45	90.56
1879–96	18	37.9	84.16	506.3	81.16	13.32	97.36
1897–1913	17	47.4	105.40	672.0	107.72	14.19	103.69
1914–29	16	58.7	130.54	827.6	132.66	14.09	103.00
1930–43	14	55.2	122.76	787.3	126.20	14.22	103.90
1866–96	31	32.0	71.00	418.5	67.08	12.96	94.68
1897–1936	40	52.9	117.64	739.6	118.56	13.97	102.06
1937–43	7	57.4	127.61	871.7	139.73	15.30	111.83

AN EMPIRICIST'S SYSTEM OF THE SCIENCES

By GUSTAV BERGMANN

A PHILOSOPHER who presents his views on the systematic interrelations between the sciences is often expected to begin by indicating, at least in broad strokes, the philosophical position upon which his views on science are based. If I were to follow this pattern, I would first have to answer such questions as these: What is modern empiricism? Is its ontology realistic or idealistic? Is its epistemology rationalistic like that of Descartes and Kant, or empiricistic in the sense of Berkeley and Hume? Is its position to the mind-body problem dualistic or monistic? And so on and so on. Clearly, no worthwhile exposition of this kind could be given, not even in the most sweeping terms, by way of introduction to a brief, nontechnical presentation. It is therefore rather fortunate that a contemporary empiricist can dispense with such a "philosophical" introduction so long as he restricts himself to some of those topics which it has become customary of late to discuss under the heading of the philosophy of science. For it is a result of the empiricist analysis of the epistemological problem that this analysis itself is quite irrelevant to the methodological understanding of science. In this respect modern empiricism finds itself in harmony with the intellectual temper of our age and its tendency to exalt science at the expense of philosophy. However, contemporary empiricists refrain from the extreme formulations of those who thought that nothing could be saved from the apparent wreckage of philosophy except mathematical logic and the philosophy or, as they prefer to call it, the methodological analysis of science. Philosophy or, to make one more verbal concession to the lovers of well-established words, metaphysics will never die. It is true, though, that under the impact of both social changes and scientific developments philosophy also changes its form. It seems thus safer to say that modern empiricism in its fight against the classical frame of reference has not so much succeeded in exterminating metaphysics once and for all, but rather that it has tried to supplant older and by now outworn ways of philosophical speculation by its own characteristic emphasis on mathematical logic and language analysis on the one hand and its own anti-ontological theory of knowledge on the other. In this attempt modern empiricism has been very successful indeed if one is to judge from the impact of its conceptions upon contemporary thought, both scientific and social. But lest all this sound overconfident, let me add that I, for one, am firmly convinced that modern or, as one also says, logical or scientific empiricism will in turn not escape the fate of all historical phenomena, the fate, that is, of being devoured by its own children.

Historically speaking, scientific empiricism can be traced back to three roots, one American, one British, and one Continental. The American root is pragmatism, as represented by such men as Peirce, Mead, James, and Dewey. The British group is that of the Cambridge analysts centered around G. E. Moore, who led the revolt against idealism in England, and Bertrand Russell. Among the Continental thinkers, finally, who were widely scattered over the geographical expanse of Europe, the best-known group is the so-called Vienna Circle, whose members are also spoken of as logical positivists. The most significant earlier figure in the European thought movement was probably the physicist-philosopher Ernst Mach who brought the anti-Kantian movement, which had been started by Helmholtz, to its first fruition. Relatively well known among scientists are also the French mathematician-philosopher, Henri Poincaré, and the Englishman, Karl Pearson, the author of the once-famous *Grammar of Science,* whose views show a rather close similarity to those of his contemporary, Ernst Mach.

But this is not a paper on the theory of knowledge or on the history of philosophical ideas. The main point here is that, in order to understand completely the logical and methodological structure of science, one does not need to delve below the level of common sense and ask such questions as: "What is

a physical object?"; "How can the investigating scientist be sure that there is anything at all outside his consciousness?" To say the same thing positively, in the philosophy of science we can take it for granted that we know the meaning of such statements as: "There are seventeen chairs in this room"; "The pointer of this instrument is at rest or has just moved to the scale point inscribed '5,'" and so on. In such a crude and sketchy formulation the phrase "knowing the meaning" may be considered as equivalent to the phrase "knowing how to verify." Statements as simple as those which have just been mentioned for the sake of illustration are, of course, verified by the scientist's direct observation of simple objects or, somewhat more accurately, of physical things and certain qualities of them which are open to immediate inspection, such as color and hardness, including certain equally simple relations on the qualitative level such as mutual position, motion, and temporal succession. Even these few remarks will suffice to indicate what one means when he says that the empiricist analysis of science starts from the level of common sense. Let us now consider some of the consequences of this apparently trivial assumption.

If all the scientist can verify are statements about physical things of the simple type indicated, and since we obviously shall not want to include into any science statements which are unverifiable, how then, is it possible to make scientific statements about such "non-things" as electrical currents, magnetic fields, gravitational forces, atoms, electrons, protons and, on the other end of the traditional system of the sciences, about human motives, thoughts, perceptions, and cultural phenomena in general, on the group as well as on the individual level? Looked at in this way our starting point, which appeared to recommend itself as mere common sense, seems to be highly restrictive, very one-sided indeed, or, at best, heavily biased in favor of the so-called physical sciences.

Before proceeding, I should like to call attention to the manner in which the traditional division between physical, biological, and behavior science is understood in this paper. The physical sciences just mentioned comprehend physics and chemistry; the behavior sciences are understood to cover such a wide range of relative complexity as, at the one end, the behavior of animals in a learning experiment and, at the other, the socio-psychological phenomena of man and his society, including the arts and belles-lettres. The biological sciences occupy the rest of the field, that is, an area which is conveniently visualized as intermediate between physical and behavior science. Whether any such division of the sciences is a hard and fast one, of basic methodological and, as some earlier philosophies maintained, even metaphysical significance, or whether it is merely a classificatory device within a unified structure, to be explained historically and justified only as a matter of expediency in view of the necessary division of labor among investigators, is one of the main questions every philosophy of science has to answer. Empiricists very vigorously assert the latter of these two alternatives and maintain what is called, ambiguously as shall be seen presently, the thesis of the "unity of science." This position even found expression in the title of one of their most representative publications, the *Encyclopedia of Unified Science*, a series of monographs published, since 1938, by The University of Chicago Press.

Methodological structure of the physical sciences. It seems advisable to restrict our problem for the moment and to leave the biological and behavior sciences out of consideration. In other words, I shall first speak about the methodological structure of the physical sciences. Since these sciences are not only practically most successful but also theoretically and historically the most mature and advanced, it stands to reason that from their study we shall obtain some valuable hints concerning the task and nature of science in general. With respect to the physical sciences the problem then stands as follows: How can our narrow criterion, according to which all the scientist verifies are simple statements about the positions of pointers and the shapes of instruments, be reconciled with the obvious fact that physicists very confidently and successfully use such abstract things as 'electrical field' and 'elasticity coefficient,' and such theoretical conceptions as 'atom' and 'probability wave' which no-

body ever expects to see or touch like physical things. It will be noticed that a further distinction has just been introduced by referring to 'electrical field' as a relatively abstract, and to 'atom' as a theoretical, concept. The two terms, 'theoretical' and 'abstract,' have not been used at random.

What is the meaning, according to the empiricist criterion, of the abstract terms such as 'electrical field,' 'electrical current,' and 'elasticity coefficient?' These terms do not directly refer to physical objects and their immediately observable properties. There is thus still a problem left. But let us now ask under what conditions a physicist asserts the presence of an electrical field around a conductor or of a current in a wire. The wire and the conductor are indeed physical objects, and statements about their motion, for instance, are of the simplicity and concreteness which our meaning criterion requires. At this point the idea and the direction of the empiricist analysis become clearly visible. Physicists say that a conductor, that is a certain physical thing, generates an electrical field if, and only if, under specifiable conditions, something directly observable happens to other physical things in its neighborhood. The electrical current, in turn, is recognized, as one usually says, either by the movement of a magnetic needle in its neighborhood, or by the heating of a resistance, or by the sedimentation of a solid substance out of a so-called electrolytical solution. Customarily one speaks of these three alternative, immediately observable events as effects by which the presence of the electric current is recognized; logically, however, this is putting the cart before the horse and one should rather say that what is meant by the presence of an electrical current in a wire is, first, that whenever one of the three alternative measuring instruments, magnetic, thermic, or chemical, is put into its circuit, the corresponding phenomenon will be observed and, second, that whenever one of these tests comes out positive, then the other two will also show the expected changes. This kind of analysis is what empiricists refer to as the "reduction of an abstract term to the physicalistic verification basis." Put this way it becomes again plausible enough that even the most abstract terms of the physical sciences, such as 'entropy,' can be thus reduced. Soon it will also be seen why such theoretical terms as 'atom' or 'electron' have so far not been considered. Their reduction requires an additional step which does not occur in the reduction of the so-called abstract terms or empirical constructs.

It has probably been noticed that in order to verify a statement which contains an abstract term the physicist must perform certain operations, in the case of the electric current, for instance, the appropriate manipulation of the measuring instruments. Such operations, however, are again specified in terms of the immediately observable thing level (physicalistic verification basis). It so happens that the empiricist thesis concerning the status of abstract terms or empirical constructs is best known under the name of "operationism," a term suggested by the manipulations just mentioned. What it asserts can now be expressed in the following manner: In order to have scientific meaning, an abstract term must be operationally introduced from the physicalistic verification basis. The terms 'introduced' and 'reduced' are here obviously used as correlatives.

A physicist, Prof. P. W. Bridgman of Harvard, has popularized the term 'operationism' through his book *The Logic of Modern Physics* (1928). But Bridgman was also careful to point out that operationism hardly deserves to be called a philosophical thesis since scientists, without necessarily being articulate about it, have always practiced operationism. As far as the experimentalist is concerned, this seems a conservative assertion. Bridgman himself drew his inspiration from Einstein's analysis of the concept of simultaneity. This contribution of relativity theory is indeed most conveniently characterized by means of the operationist's terminology. For Einstein has pointed out that the assertion of the simultaneity of two events which are too far apart to be simultaneously observed by the investigator is not a statement simple enough to go unchallenged from our viewpoint and is, therefore, in need of operational reduction.

Concepts and laws. The principles of

methodologically correct definition of abstract terms or concepts are, indeed, all operationism is concerned with. Historically and psychologically speaking, the creation of helpful concepts is, of course, a very essential part of any scientific achievement. But let it also be noticed that if a concept is correctly defined, it is therefore not necessarily helpful. A person's weight multiplied by the number of his hairs and divided by the third power of his blood count is a correctly defined empirical construct, and still one can safely predict that it will never receive any attention in science. The reason for this is that there are no known empirical laws which connect this rather gratuitous creation of my whim with other concepts, as pressure, volume, and temperature are connected in the formula of Boyle and Charles, or which would assert its constancy, as Newtonian mechanics asserts the constancy of mass. Systematically, however, this way of speaking again puts the cart before the horse. A concept appears questionable to us if, and only if, there are no known laws about it; and it seems even gratuitous or absurd when we do not expect ever to find such laws. Expectations of this kind are, of course, nothing absolute but merely a rather general frame of reference or a thought habit dependent upon the laws actually known at any given moment in the history of science. Much of what scientific empiricists object to in traditional philosophy are really attempts to legislate in an *a priori* manner as to the necessity, whatever that might mean, of such frames of reference.

Concepts or empirical constructs are merely the tools of science. What these tools are supposed to produce, through systematic, theory-guided experimentation, are the *empirical laws*, that is, the functional relationships between the concepts or, as one also says, variables defined. To say the same thing differently: Empirical laws are found or hypothesized by means of inductive generalization from observation and experimentation.

Scientific laws have something to do with causality and induction, so one might suspect that at this point, in his conception of the nature of scientific laws, the empiricist brings in some of his epistemological views and thus surreptitiously violates our agreement not to descend beneath the basis of common sense which is shared by all, irrespective of philosophical positions. However nonempiricist philosophers might feel on this point, a strong case can be made that nothing of the kind is being done as far as science is concerned. For to the scientist it is obvious that extrapolation from the tested to the untested case, prediction of future unobserved, on the basis of past observed, regularities is, in a certain sense, but a guess, no matter how successful in practice, or, to use Hume's illustration, that in a philosophical sense it is neither certain nor necessary that the sun will rise tomorrow. For the scientist, if not for the man on the street, this is again just common sense followed through to its logical conclusion. Technically this fundamental feature of our knowledge is referred to as the hypothetical or inductive character of all empirical laws.

With respect to causality the situation is not much different, though somewhat more interesting in view of recent developments in physics. One can often hear it said that modern quantum theory, in replacing the causal laws of classical physics by mere probability correlations, has brought about a major revolution in our philosophical ideas. A major revolution it was indeed, but only within science, not in philosophy, at least not as far as the empiricist is concerned. From his viewpoint *causal law* does not say one iota more than *law*, and finding the causes of events means for him nothing more and nothing less than finding the laws or regularities governing their occurrence. For the scientist at least this is again just common sense. A law in turn is, logically considered, a statement of the form, 'If A then B,' or rather, 'Whenever A then B,' the only requirement being that all the expressions which occur in 'A' and 'B' are operationally defined. For if they were not, how could we test the law or even know what it means? But it makes no difference whether the regularity which the law asserts obtains between so-called individual events or between statistical frequencies. What, for that matter, is an individual event? Frankly,

this question is much too difficult to be tackled here.

Scientific theory and explanation. So far we have gained some insight into the two notions of *scientific concept* and *scientific law*. Our attention must now turn to the ideas of *scientific theory* and *explanation*. A scientific theory (high-order explanation) can be described as the logical integration and organization of a whole body of empirical laws. Scientific understanding is the result of scientific explanation. To be specific, when physicists found the three formulae or laws of free-falling bodies, of the inclined plane, and of the pendulum respectively, they also explained these phenomena (low-order explanation). But their understanding was improved when these three empirical laws were shown to be special cases of one set of more general laws, the laws of mechanics. Again, Newton reached a still higher level of explanation when he derived the phenomena of two so far entirely unrelated fields, astronomy and mechanics, from one and the same set of basic laws. But one must not forget that we call these laws more general or more basic only because they allow for such logico-mathematical derivations of other laws. The psychological effect, the increase in understanding attained, is, of course, still more marked if such integrations allow for the prediction of laws so far not even known in isolation. Then we feel that we "really understand."

In a loose, but suggestive manner of speaking, one might say that empirical laws integrate and predict individual facts while theories, in the broader sense of the term, integrate and predict empirical laws. The process of such integration is very appropriately described as a process of unification and so we gain, within the field of the physical sciences, a first glimpse of what could be meant by the unification of science. This process and its results will be referred to as *unification as to theory.*

Theoretical concepts. To understand the nature and function of those concepts which are not merely abstract but theoretical in the narrower sense, such as 'atom' and 'electron', consider, for instance, the two fields of mechanics and thermodynamics. Typical for the one is the Newtonian attraction formula or the law of the lever; typical for the other, the formula of Boyle and Charles, which I have already mentioned, or the formula which connects the boiling point of a liquid with pressure. In order to unify these two fields, scientists have resorted to what has been called the construction of *models*. The gas in particular is thought of as consisting of a great number of particles which are subject to the laws of mechanics. What we observe as gas pressure is said to "correspond" to the total mechanical impulse of those imaginary particles; temperature in turn is specified as some function of their average velocity, and so on through the whole list of the empirical constructs of thermodynamics. To each of them a mechanical property of those imaginary little balls is *coordinated*. The idea of the whole game is that after the coordination has once been made, one expects to derive the thermodynamic laws as special cases from the laws of mechanics by applying the latter to the imaginary particles. If this should work, as it actually does or did within very wide limits, a tremendous step towards unification has been made. Everybody realizes, of course, how sketchy a description of the so-called classic kinetic theory of Maxwell and Boltzmann this is, yet it will serve our purpose. So let me start on a series of four brief remarks of a more general nature.

First, it should be stated that by this so called mechanical model the operational definition of the empirical construct 'temperature' is not in the least affected, and we also still experience heat as heat. So there need be no fear that the "mechanical" theory of heat has impoverished the world by denying it the warmth and glow, in this case quite literally, of all nonmechanical qualities. This is a useful point to remember whenever one comes across certain very fashionable arguments that science, particularly in the domain of consciousness and culture, impairs the appreciation of the totality of human experience. The situation in these fields is admittedly much more complex, but there is no difference in principle. By explaining, if necessary even in physiological terms, the

occurrence of a value experience, one has neither denied its occurrence, nor challenged the value in question, nor, finally, stripped the world down to the skeleton of a meaningless machine.

Second, one must realize that even when theoretical unification has been achieved, that does not mean that one always has to go down to the rock-bottom level of the most comprehensive scientific explanation. To give an illustration, the engineer who uses the laws of thermodynamics in designing a steam engine does not need to concern himself with the derivation of these laws from the kinetic model or, to borrow an illustration from the biological field, the neurosurgeon will not gain any additional skill by tracing down to the level of the electric reactions of the nerve fibers the causal mechanisms he utilizes. Again, no reasonable person will expect the clinician to analyze the personality traits of his patients down to that level of elementary conditioning which is most pertinent in learning experiments. But one must not forget either that theoretical reductions or unifications are not only intellectually very satisfying but sometimes do lead to the discovery of new empirical laws which prove extremely useful to engineers, surgeons, and clinicians.

Third, it was to such imaginary models as the atom that the term 'theoretical' in its strieter meaning has been reserved, while 'empirical construct' was used for concepts, no matter how abstract, which do not contain any reference to such imaginary things. To put it differently, we want to distinguish between imaginary things and non-things. The problem involved is best understood if one tries to answer the naïve question whether there are really atoms, whether atoms are real, and if not, how an empiricist could justify these brain children of the scientist. One answer at least to this famous question goes about like this: Whatever the philosopher has to say about the meaning of the word 'real,' both scientific and plain common sense require that it must not be interpreted in so narrow a sense as to comprehend nothing but physical things like chairs, stones, and apples. 'Real,' in other words, is not synonymous with 'material object.' Philosophically speaking, there are not only things in the world but also their manifold patterns, structures, and relations. Speaking in terms of physical science this means that forces, electrical fields, and elasticity coefficients are as real as the physical objects which, as we saw, form the operational basis for their definitions. 'Real,' briefly, is for the scientist everything that finds its place in the hierarchy of the empirical constructs. Models, as the gas model described, obviously do not occur in the hierarchy of operationally defined concepts; they are later on coordinated to empirical constructs and are, in a certain sense, merely computating devices or, if you please, *unifying devices* to establish logical connections between various classes of empirical laws. The way the question of the reality of the theoretical particles is often put and treated is, therefore, about as meaningful as to ask whether the Mississippi is male or female. It simply is not a good question and, to use the poignant phrase of one of the most influential living philosophers, "Where there is no question there is no answer."

Atomic theories are probably the most conspicuous technique of explanatory unification. There are, however, other unifying devices. The unification of optical and electromagnetic laws for instance or, as one usually says, the recognition of light as an electromagnetic phenomenon, the great discovery which we owe to the genius of H. Hertz, lies entirely within the realm of the empirical constructs. The reduction of this "theory" requires an analysis of its own.

Fourth, I want to emphasize that the so-called indeterministic character of modern physics lies entirely in the realm of the model. Galton already has called attention to very simple mechanical systems about which no other but statistical predictions can actually be made. The point is that in spite of this we used to refer everything, at least in principle, to a universal atomic model between whose elements laws of the Newtonian rather than of the statistical frequency type obtained. The resulting difficulties which finally led to the abandonment of the model have sharpened our insight into the purely fictitious or calculational charac-

ter of models in general and, in addition, have helped us to clarify, rather than to revolutionize, our notion of scientific lawfulness. As to the claim which hides itself behind the phrase "in principle," we have learned to recognize it as an empty verbalism wherever it pretends to go beyond the limits of the operational basis.

The system of the sciences. We are now ready to examine what insight we have gained so far into the *system* of the sciences. Are there physical sciences, different theories, on the one hand, and concepts and empirical laws of different categories on the other, or is there, if one considers the matter from the vantage point of the philosophical methodologist, only one physical science, one unified body of theory; is the logical nature of all kinds of empirical constructs and empirical laws the same? If the problem has once been focused this way, it becomes apparent how ambiguous the issue of the unity of science is and how easily therefore its discussion can lead to confusion, if one approaches it without preparatory analysis of the kind here indicated. One really has three questions on hand, one as to *concepts,* one as to *laws,* one as to *theories,* and the answers to them are not necessarily either all positive or all negative. The last question, as to unity of theory, is not even of the same general character as the first two concerning concepts and laws. More specifically, unity as to theory is entirely a question of fact, that is, of actually achieved theoretical unification, while the question about the status of laws and concepts allows for an answer of comparatively much greater or, if you please, philosophical generality. And now for the empiricist answer itself. According to it there is only one legitimate way of defining scientific concepts, the way of operational definition from the physicalistic verification basis; and there is only one way of analyzing any empirical law whatsoever, the analysis into a universal if-then correlation ('Whenever A then B') between empirical constructs. As to theory, the extent to which, through coordination of models or through other logico-mathematical devices, these laws can actually be integrated into a unified body of deductive consequences from a relatively small number of so-called basic laws, cannot be determined on logical or methodological grounds. For physics and chemistry, though, the scientific developments of the last two generations have furnished an affirmative answer to this question. Notwithstanding the many problems which are the concern of the specialist, the theoretical or the explanatory unity of physico-chemistry is today a fact. This being the case, one might wonder why I spent so much time on the discussion of an area which is already organized, instead of concentrating on the biological and behavior sciences which, since they are less accomplished, may stand in greater need of philosophical clarification. To this I should answer that only by studying the structure of physical science can one provide himself with the instruments which will guide him through the metaphysical fog which, partly because of our relative ignorance, partly because of what sociologists call the cultural lag, still sometimes beclouds a clear methodological understanding of the nonphysical sciences.

The phenomena of purpose and consciousness. The empiricist position on these much more debated issues is that, in order to be of scientific significance, all concepts, no matter where they occur, must be operationally defined in terms of the physicalistic verification basis; that science, whenever it tries to explain or understand, must look for empirical laws, and that therefore all scientific laws, no matter in which field they occur, are of the type of a universal if-then correlation, for otherwise they could not allow for prediction. In other words, as far as laws and concepts are concerned, the methodological unity of science is for the empiricist again just common sense, though it is admittedly not common sense for many a contemporary student who adheres to one of the older philosophical systems. Though the objections raised against the empiricist thesis are very variegated, the whole cluster of them is well represented by the belief that both the concept of purpose and the concept of consciousness defy any attempt at reduction to the physicalistic verification basis. The objection of purpose already arises in the biologi-

cal sciences, that of consciousness is most frequently voiced in connection with human behavior

Before taking up these objections, let us recall that we are not here concerned with epistemological analysis below the common sense level. All we have to do is to answer the question: under what conditions does the scientist describe the behavior of an organism as purposive or attribute conscious states to one of his human subjects? Sometimes it is factitiously alleged that modern science denies the *phenomena* of purpose and consciousness, and that modern empiricism, abetting it, tries to furnish the philosophical justification of such outrageous nonsense. In matter of fact, modern science does not eliminate or deny purpose and consciousness any more than the kinetic theory eliminates or denies the phenomena of heat. As an *epistemology* modern empiricism deals with purpose and consciousness in its own manner, which one could very, very roughly characterize as a refinement of Hume's approach; by doing so it puts itself into a position to appreciate, as a *philosophy of science* or on the common-sense level, the meaning of the question that has been formulated a moment ago: What does the scientist mean by purpose and consciousness? This again, as we have learned from the discussion of physical science, is equivalent to the question: How does the scientist recognize their occurrence and how do these concepts enter into laws which lend themselves to the only purpose of science, namely that of prediction?

To be sure, then, purposiveness or, to use a less anthropomorphic expression, adaptability is one of the most outstanding features of organisms. The exploration of whatever can be said about the *purpose of Nature* the scientist as well as the empiricist may safely leave to the old-style metaphysician, but they will certainly have to account for the occurrence of purpose in nature; that is, for the goal-directed behavior of those physical objects which we call organisms. Everyone even slightly familiar with modern psychology knows that at the present stage of our knowledge the most direct way to the exploration of this field leads through the systematic study of the learning process. There one finds, on the common-sense level, such statements as that organisms learn or act purposively toward the attainment of a goal, if, and only if, their motivation is directed towards that goal. But let us ask ourselves whether this formulation, the way it stands, is an empirical law, or, to put the same thing differently, whether it has any predictive value. All it says, it seems to me, is this: whenever the organism does show certain kinds of objectively describable behavior towards certain objectively describable states of affairs or objects, then we say that it has been appropriately motivated; if not, not. But this is clearly no more than a descriptive account, in empathetic terms, of what has happened, *after* it has happened, not a prediction of what is going to happen. What the scientist tries to find, in order ultimately to control them, are, therefore, just those objective factors in the organism's past and present environment which elicit that behavior which we then, in prescientific language, call purposive or ascribe to the organism's motivation. Only on the basis of such knowledge can one predict; that is, after these factors have been found, laws can be formulated and this without any use of such terms as 'purpose' or 'motivation.' In other words, this paragraph itself is an informal sketch of the actual reduction to the physicalistic verification basis of the two crucial and allegedly irreducible concepts. In technical terminology 'purpose,' 'motivation,' and the like, turn out to be so-called dispositional concepts, the elimination of which does not restrict the expressive potentialities of our language.

Turning now to the notion of consciousness, all one has to find out is again under what conditions scientists or clinicians speak about conscious states of their subjects. And again the answer is sheer common sense. All we have to go by if we ascribe consciousness to a subject, or, to say the same thing differently, if we use mental terms, such as 'feeling,' 'knowing,' 'believing,' in the description of his behavior, is this behavior itself. Behavior, however, including speech behavior, can be described in terms of the pysicalistic verification basis. This aspect of the empiricist position is best known under the name behaviorism. But to say anything worth while about behaviorism would require

at least another article of the same length. So I shall content myself with the mere reference to this well-known position which is accepted by all contemporary psychologists with the sole exception of the small German-influenced group of the Gestalters.

Vitalism and mechanism. While the empiricist thesis in the behavior field has been labeled as behaviorism, a similar tie-up with well-known positions in the biological field has been omitted. The labels which here come to mind are, of course, those of mechanism and vitalism. On first sight it might appear that the mechanistic thesis is an integral part of the empiricist position. Such belief would be mistaken. The situation requires further analysis in the light of the threefold schema of concepts, laws, and theories. It is true that vitalists and related writers often take their stand on the alleged irreducibility of the concept of purpose and, in connection with this, assert the existence of so-called teleological laws. Nothing more needs to be said about the alleged irreducibility of purpose. As far as teleological laws are concerned, purely logical analysis reveals that this unclear notion cannot even be precisely formulated. Thus a question of fact never arises. With respect to unity of theory, however, the thesis of vitalism can—irrespective of its truth or likelihood—at least be formulated in a manner which makes sense both to the scientist and to the empiricist philosopher.

This brings us to the issue of a theoretical unification of all three levels: physical, biological, and behavioral. Clearly and in contradistinction to the situation within the physical sciences, such unification has as yet not been achieved. Even within each of the two higher levels, we do not yet possess comprehensive bodies of empirical laws, or at least not bodies of laws the significance of which we could trust in the theoretical sense that they are certain to provide the cues for a potent and comprehensive theory after the manner in which Galileo's findings provided a basis for Newton's theoretical achievements. As long as this is the case, actual explanatory unity as to theory between all the three levels is *a fortiori* impossible. The point is rather that philosophical mechanism and philosophical vitalism both defeat their own purposes by furnishing *a priori* arguments for matters of fact, the one for the impossibility, the other for the necessity of an ultimate unification as to theory.

To mechanism one must grant this: as to laws and concepts, the unity of all science is a fact, not a program. If this is once clearly understood, the props are indeed knocked out from under the better part of the vitalistic speculation and from under the pronouncements of the Gestalters. To vitalism, on the other hand, one must grant that much: it is perfectly conceivable that in its attempts to describe the physical processes in those tremendously complicated systems which we call organisms, scientists will discover new physical laws which are not theoretically reducible to the already known basic laws of physics and would, therefore, have to be added to them. It is, however, not the case that the mere occurrence of the phenomena of purposive behavior and of consciousness justifies such an expectation. Quite to the contrary, the whole frame of reference of modern science, the vigorous growth of such disciplines as biophysics, biochemistry, physiological psychology, and the theory of learning strongly indicate the improbability of what has here been exhibited as the formulable empirical core of the vitalistic contention. In this sense mechanism may indeed be said to be an essential part of our scientific and cultural frame of reference. But the modern analytical philosopher is neither the ideological defender nor the fact-finding supporter of such frames of reference. His job is to sharpen their formulation and, by doing so, to clarify their meaning. In the case of vitalism versus mechanism the result of the analysis is that we are here not faced with a philosophical issue *about* science and still less, as some factitiously contend, with a cultural issue, but rather with a very vague negative prognosis concerning the result of future research *within* science. Since the general cultural context has thus come up again, it will, I hope, not be inappropriate to conclude by expressing the conviction that the intellectual caution, care, and cleanliness upon which empiricism insists in such analyses, is itself a major cultural value.

AIR-BORNE SPORES AND PLANT QUARANTINES

By W. A. McCUBBIN

In estimating the ultimate worth of plant quarantines as a means for the permanent exclusion of injurious plant diseases from the United States, it is necessary to take carefully into account the possibility of spore dissemination over long distances in air currents. When it is considered that spore dissemination by air is entirely uncontrollable and that air movements on a large scale are a universal and constant feature of nature, the question may be raised: Is natural air transport effective enough to lessen materially the value of these quarantine efforts, at least for some diseases? Or does long-distance air transport of spores involve such difficulties and limitations that it may be largely ignored in planning plant quarantines?

In attempting to review the essential elements of this problem we are confronted at once with the difficulty of trying to maintain in correct perspective in a single mental picture two such extremes of size as our huge planet and a microscopic spore; fortunately, this mental feat can be made somewhat easier by some preliminary discussion of each extreme.

We unconsciously tend to regard the air overhead as a limitless expanse, extending almost indefinitely above us into space. This vague idea serves well enough in ordinary affairs, but when we attempt to grasp the conditions attending the air movement of spores, it is necessary to obtain a more accurate concept of our earth's atmospheric envelope.

On a planetary scale the terrestrial air blanket consists of only a thin and scanty covering of gaseous materials held tight to the earth's surface by gravitation. As air is highly compressible, its lower level is relatively dense, but the mass thins out rapidly upward. At a comparatively short distance from the surface the gaseous matter becomes so dispersed and tenuous that the air occupying the uppermost portion above about 40 miles is, from our point of view, of little importance. About 25 miles from the surface an ozone region is said to exist, which screens out virtually all the destructive short-wave components of sunlight; however, the longer light waves penetrate readily to the earth's surface, where they are reflected, absorbed, or transformed into heat. The air mass in direct contact with the earth will, therefore, be denser, warmer, and more disturbed by thermal activity, resembling in this respect the air immediately above a hot stove. A multitude of convection currents, both vertical and horizontal, abound in this lower layer, and tend to distribute the surface-generated heat. The thermal system thus set up functions somewhat on the pattern of what goes on in a kettle of boiling water.

We may get a clearer comprehension of the actual spatial relations of this terrestrial air layer by imagining our earth reduced to the size of a 12-foot sphere. On a world of this size the atmospheric blanket would extend outward at most 3.5 inches from the surface; the more compact, lower 40 miles, dense enough to be of interest in our problem, would reach only $\frac{3}{4}$ inch from the surface; the ultraviolet filtering region would begin less than $\frac{1}{2}$ inch up; the lowest 7 miles, containing practically all storm and weather disturbances, would have a ceiling of $\frac{1}{8}$ inch; and even in this thin shell most of the weather phenomena and most of the fungus-spore movement would be found in the lowest 3.5 miles, or in a thin skin $\frac{1}{16}$ inch deep. On this same scale the long stretch of plains country extending from Mexico to the Canadian border would represent approximately 20 inches; the 3,000-mile Atlantic hop, 54 inches; and the 5,000-mile expanse of the Pacific, 90 inches.

In the light of these conditions it would obviously be incorrect to visualize air-borne spores as shooting off into illimitable space like the rocket ships of fiction and eventually falling back to earth again far distant from their point of departure; rather we must picture them as trickling along in a horizontal course, almost hugging the surface, and confined in a thin lower layer of the air, where they are subject to all the disturbances and

vagaries of this most turbulent portion of the atmospheric envelope.

Turning now to the spores themselves, we find that, although they consist of vegetable matter comparable in density to apple or wheat tissue, their minute size brings them into the dimensional plane of dust particles, a plane in which certain physical relations become markedly altered. In a series of objects of the same shape and substance but decreasing in size, the volume or mass decreases much more rapidly than the area. If we recall further that the downward pull of gravity is proportional to mass, while the resistance of air to bodies passing through it depends on their area, it is apparent that as size diminishes, air resistance will have a rapid increase relative to gravitational pull. This increasing disproportion between air resistance and gravitational pull becomes very pronounced at the size level of dust and spores, so that spores tend to fall very, very slowly.

The rate of fall in still air has been determined experimentally for a number of spore types. An oval spore measuring 20×12 microns could be expected to have a falling rate such that if it were released at a height of 1 mile, 3 days would be required for it to fall to earth.

This surprisingly slow rate of fall is, of course, a basic point of departure for estimating the possibilities of long-distance transport; theoretically a 20-mile breeze could carry the spore example just cited 1,440 miles across country while it was falling to earth from its mile-high point of release.

Such calculations of rate of fall, however, do not themselves bring us to any final conclusions in the problem of long-distance spread. They merely serve to establish its possibility, and particularly to convince us that, because of this feeble tendency to fall, fungus spores in general are capable of being transported in favorable air currents to almost any distance over the earth.

Having thus touched on the salient characters of the air as a carrying medium and the fungus spore as a particle to be transported, we may proceed to consider the vast number of plant pathogens awaiting transportation at some foreign point of origin, the conditions affecting spores throughout the course of a long air journey, and the possibility of successful establishment on their arrival at a distant destination.

Starting with the situation at the point of origin, we are able to conclude at the outset that spores of certain diseases would be airborne with difficulty or not at all. Examples include potato wart, common scab, rhizoctonia, powdery scab, and blackleg of potato, and diseases of other plants such as crown gall, club root, and many wilts. All these are so bound up with the soil that the chance of spores getting aboard air currents in appreciable numbers is negligible.

Again, many pathogens remain so immersed in host tissues that long-distance spread is almost precluded; such troubles as the Dutch elm disease, pear blight, various virus diseases, and nematodes belong here. The nontravelling class also includes a large group of fungi with dispersal arrangements primarily adapted to water dissemination. The very numerous species of *Gloeosporium* and *Cylindrosporium* are good examples. Mention must also be made of fungi that normally sporulate in such sheltered locations that it would be difficult for their spores to get into air currents. Spores sensitive to drying out and those with extremely short natural periods of longevity may also be left out of consideration for long jumps.

What proportion of disease organisms will fall into this stay-at-home group? Although no exact answer is possible, we may get some light on the general situation by the sample-analysis method. Selecting as a sample the list of 200 representative diseases already assembled for another quarantine-analysis study, it is found that, for one reason or another, from 75 to 90 percent of them appear to be incapable of more than limited or local distribution through the air. Conditions in this sample indicate, therefore, that long-distance transport as a quarantine problem narrows down to a comparatively small percentage of pathogens peculiarly adapted to this type of dissemination.

The ascension of spores into the air at the point of origin presents no difficulty. The slightest convection movements, such as are constantly present everywhere, involve velocities far in excess of the feeble falling

tendency. A barely perceptible breath of air moving at 1 mile per hour dwarfs into insignificance the falling speed of the spore previously cited, which is 72 feet per hour. Thermal convection masses, therefore, can readily carry spores upward to whatever height these masses rise, and thus bring them into favorable position for extended horizontal travel.

Horizontal travel, however, is attended by many uncertainties. Downward convection currents may carry the spore rapidly to earth again; winds die out or change direction; desiccation may fatally affect some spores; low temperatures fatal to a few spores occur at altitudes comparatively near the surface; and sunlight, known to be damaging in some cases, has a more intense effect in the upper regions, although spores are unlikely to be carried up to altitudes of 25 miles or more, where they would be subject to extremely deadly ultraviolet radiations.

Spores adrift in the upper air currents are subject to two other major obstacles to extended transport. By virtue of an acquired electric charge each spore serves readily as a nucleus for moisture condensation, so that when vapor-laden air carries a spore to cloud-formation levels and is chilled, either by mere expansion or by contact with colder upper air masses, the spore is likely to find itself locked within a water droplet. And if condensation progresses to the stage of rain, the included spore is inevitably carried back to earth in the falling drop. Observations indicate that just as rain clears the air of floating dust, it is equally effective in washing down drifting fungus spores.

High mountain chains are a second formidable obstacle to spore travel. When air masses carrying spores are deflected upward by mountains, the cold of altitudes at or near the summit is favorable to moisture condensation, which is likely to bring the spores to earth in rain, mist, or snow.

When a fungus spore finally comes to earth 1,000 miles or more from its starting point, after an erratic period of helpless drifting, it is confronted with still other difficulties. Unless it can happen upon a suitable host it is lost. For the visiting spore not only are sandy wastes, plowed fields, snow blankets, and water expanses utterly barren, but even a vast and luxuriant nonhost flora constitutes an equally hopeless prospect; in fact, any new environment may be largely an inhospitable desert, where a spore's chances for reaching small or scattered host plants are extremely minute. The chance that an individual spore would both survive extended transport and establish successful infection is so infinitesimal that distant migration would have to involve almost incredible numbers of spores in order to bring success for even a few of them within any reasonable degree of probability. One may therefore surmise that only those fungi capable of charging the air with a tremendous spore load will have any appreciable chance for successful long-distance spread.

Other difficulties beset the newly arrived spore. It may reach its destination at the wrong season, the host may be in a resistant or dormant stage, conditions for infection may be adverse, either winter or summer climate may be too severe, or a necessary alternate host may be lacking. If the fungus species represented by these spore immigrants can live saprophytically for a time, or establish a more or less permanent relation with its host, or propagate itself immediately by several rapidly produced generations, its chances for developing a slight foothold into permanent invasion are vastly increased. But spore mortality is so high and the requirements for bridging interseason gaps are usually so exacting that, unless the spore possesses some special advantage, light initial infections are likely to die out completely.

Looking at these peculiarities of behavior from the plant quarantine point of view, it is encouraging to note that, so far as long-distance spread is concerned, most plant diseases belong to the stay-at-home group. It is equally encouraging to note that the probability of intercontinental dissemination become appreciable only for fungi with a large scale initial spore production; comparatively few fungus species can qualify in this respect. The feeble rate of fall, associated with the small size of spores, assures us that spores can be carried upward with the greatest ease in even slight convection currents and that the possibility of horizontal travel in wind is al-

most unlimited. It is some comfort, however, to observe that the course of air-borne spores is relatively close to the earth's surface where the spore load is subject to constant and severe depletion in the turmoil of this thermally active contact layer. Finally, even for species that possess favorable qualifications for travel, the overseas trip is very much of a gamble, because it involves a series of uncertainties, practically all of which must come out favorably to bring success. Distant air travel by fungus spores is therefore made against heavy odds.

For easier comprehension, we may translate this general situation into a scale of more familiar terms. Imagine an extensive flat garden roofed over with glass at a height of 6 feet. An experimenter stands at one side with an electric fan and a cupful of brown rot spores which he is attempting to blow toward ripe peaches on a tree about 300 yards away, in the hope of infecting them. Stretched across the garden midway to the peach tree is a 5-foot privet hedge and an overhead sprinkler system at work. The chance of any of his spores infecting the peaches under these conditions represents in a rough way the chance of a spore's making a successful jump of 1,000 miles in air currents over the earth's surface.

It is apparent, therefore, that against the relatively small number of foreign fungus species endowed with all the requisites for successful dispersion by air, our chief protection lies in this very matter of probabilities. It is possible that any of these species could be lucky throughout the whole series of events en route, though ordinarily their chance of success is probably very small. In any case, the slight possibility of disease introduction by air is judged to be far too small to affect materially the worth of the plant quarantine effort, the primary concern of which is to prevent disease and insect introduction from other world regions overseas by establishing a watchful supervision over the controllable channels of human travel and commerce.

FEDERAL PLANT QUARANTINES

Before

The United States is the only great power without protection from the importation of insect-infested or diseased plant stock. . . . A properly enforced quarantine inspection law in the past would have excluded many, if not most, of the foreign insect enemies which are now levying an enormous annual tax on the products of the farms and orchards and forests of this country. . . . In addition to the danger of importing these insect pests is the risk of bringing in new and dangerous plant diseases. . . . The necessity for a national quarantine against foreign insect pests and plant diseases has long been recognized, and during the last 14 years, especially, a strong continued effort has been made to secure such legislation. . . . The first concerted effort to obtain a national quarantine and inspection law was due to the introduction in the East on nursery stock of the San José scale in the early 90's. The failure to reach an agreement as to suitable legislation among the nurserymen, fruit-growers, and entomologists prevented anything coming from this effort, although several bills were introduced in Congress from time to time. . . . The present effort to secure legislation resulted from the discovery two years ago of the introduction of enormous quantities of brown-tail moth nests, full of hibernating larvae, on seedling fruit stock, chiefly from northern France.

After

The need for national quarantine legislation for the protection of the farm, garden, and forest interests of this country from further invasions by foreign pests was long appreciated, but the securing of this legislation necessitated an extended period of earnest effort. Toward the end of this period this legislation was hastened by the increasing numbers of gipsy and brown-tail moths found during the years 1909–12 on imported plants, and also by many other insects and plant diseases and by the need, which began to be generally appreciated, of excluding such other important pests as the Mediterranean fruit fly, the potato wart, and the white pine blister rust. The movement was aided also by the experience with the chestnut blight and the San José scale. The entry of the citrus canker, the Japanese beetle, and the European corn-borer all occurred during the last years of the effort to secure this legislation and before it was actually enacted, although the fact of the entry and establishment of these pests was not determined until several years later. The Plant Quarantine Act of 1912 was the final outcome of this 14-year effort to secure authority to protect the United States, so far as possible, from further entry of plant pests. In connection with broad quarantine and regulatory powers, this act makes specific provision for the regulation of the entry of nursery stock and other plants.

By C. L. MARLATT, "the father of the Plant Quarantine Act of 1912." Quoted by permission of the National Geographic Society. Left, from the *National Geographic Magazine*, Apr., 1911; right, *ibid.*, Aug., 1921.

SCIENCE ON THE MARCH

ETNA CAVE, NEVADA

WORK in various phases of the problem of the pre-Columbian occupation of the southwestern United States has practically come to a standstill as a result of the war. Information pertaining to investigations carried on prior to the outbreak of hostilities is gradually becoming available, however, and serves to stimulate continued interest in the subject. One of the lesser known, although significant, archeological projects in this category was that at Etna Cave, located about 6 miles south of Caliente in Lincoln County, southeastern Nevada. Excavations in its deposits demonstrated that it had been inhabited at different times by various groups extending from the late developmental period of the Pueblo peoples—the stage just preceding the era when the large communal houses and cliff dwellings whose ruins dot the Pueblo Plateau were built—back to the days when the native horse, camel, and giant sloth roamed the countryside.

The cave is situated in a narrow canyon, lies within a wall of volcanic tuff at the top of a short, steep talus, and faces southeast. Its entrance is protected by an overhanging cliff which also extends some distance towards the southwest and forms a shelter area along the top of the talus. Mr. J. B. Tennille, a prominent resident of Lincoln County, originally called the attention of Mr. S. M. and Mrs. Georgia Wheeler, then located at the Panaca, Nev., CCC camp, to the formation and its archeological manifestations. The Wheelers made a preliminary inspection, did some testing, and reported their findings to officials of the Southwest Museum, Highland Park, Los Angeles. This led to arrangements between the Southwest Museum and the National Park Service for a program of excavations at the site. Boys from the Panaca CCC camp were used for the work. The digging was carried on under the general supervision of Mr. M. R. Harrington, curator of the Southwest Museum, with Mr. Willis Evans in direct charge. Most of the formerly occupied area was uncovered by that group, although a small section left to serve as a control was completed two years later by a party from the Southwest Museum.

The cave showed a sequence of temporary occupations, but the shelter only gave evidence of use by the Pueblo peoples. The excavations indicated that in the beginning the cave had been visited at frequent intervals by a nomadic hunting people who made it a camping place rather than a permanent habitation. It is not known who these people were. Their weapons and tools, however, suggest that they may have been contemporaries with other early hunting groups—such as the so-called Folsom men and the occupants of Ventana Cave in southern Arizona—whose material culture is found in association with the remains of extinct species of animals under conditions indicating a terminal Pleistocene or beginning Recent geologic age. For some reason these visits ceased. There was an interval during which the accumulation of refuse was covered by a natural layer of dirt and debris that was gradually tramped down by the feet of animals until it formed a hard-packed "floor" throughout the cave. There is no scale by which to determine how long a period this unbroken stratum represented, but it probably was of considerable duration.

Other wanderers drifting into the area then came upon the cave and a new cycle of inhabitation followed. Artifacts found in the layer formed from their debris indicate that they were related to the people who once lived in Gypsum Cave, located in a spur of the Frenchman Mountains east of Las Vegas, Nev., about 100 miles to the south. Previous work at the latter location by Mr. Harrington had revealed that the oldest inhabitants there had hunted the ground sloth (*Nothrotherium shastense* Sinclair), camel (*Camelops* sp., and *Tanupalama* sp. of which two species are probably represented), and possibly were acquainted with the horse (*Equus* sp., a small form, and *Equus* sp., a large horse, cf. *occidentalis*). Contemporaneity between the artifacts, the ground sloth, and the camel was demonstrated, but there has been some

doubt concerning the horse. Other caves, such as Smith Creek Cave and Upper Baker Creek Cave in White Pine County, Nev., some 100 miles north of Etna Cave, also indicated that these early hunters might have known the native horse yet did not establish that fact. In Etna Cave there was evidence for such contemporaneity. No horse bones were found, but in the level containing the artifacts similar to those from Gypsum Cave were scattered feces that have been identified as horse dung. Analysis has shown that they were a winter product from a small animal or animals that had been feeding on yucca, brushwood, and other unsucculent food. It appears that the horse sought shelter there during an interval when its intermittent human tenants were elsewhere, although they subsequently returned before their ultimate disappearance from the district. Hence it is logical to suppose that they must have seen occasional horses even though they had no direct contact with them. Were it not for the overlying strata and their archeological content the traces of horse could be attributed to post-Columbian times. Evidence from the subsequent deposits, however, is against such an explanation. It must have been a native and not a European horse. The top of this layer was covered by a "floor" similar to that capping the bottom level.

Above the second "floor" was a layer of dust, debris, quantities of dried grass, shredded juniper bark, yucca leaves and fibers, corncobs, and other vegetal material. It contained numerous artifacts and discarded articles that identify the horizon as that of the Basket Makers, the group of people preceding the Pueblos in the Southwest. Two phases were represented in the layer. In the lower portion the objects were indicative of the true Basket Makers, while the upper contained materials indicating progression to the late or Modified Basket Maker stage when pottery making and increased agricultural activity had become a part of the culture. The characteristics of both phases of the Basket Makers are well known from work in their remains in the Four Corners area of Utah, Arizona, Colorado, and New Mexico and their relative ages have been established. They antedate the Spanish pentration of the Southwest by 700 to 1,000 years. This material was also sealed in by another of the hard-packed "floors."

An appreciable lapse of time seems indicated by this upper "floor," both in its formation and a subsequent period of disuse, because the cultural material occurring in the layer above is characteristically late developmental Pueblo, Pueblo II as it is sometimes called, in form. There was no early developmental Pueblo horizon in the deposits, hence the break must represent a minimum of several generations or, what is more likely, one to two centuries. The absence of early developmental Pueblo manifestations is a condition commonly found in Nevada and probably is attributable to the fact that it was peripheral to the main centers of Pueblo growth and was slow to feel their influence. It was not until the cultural pattern had become firmly established in nuclear portions of the Pueblo area that it began to diffuse westward and by the time it reached eastern Nevada was in the late stage of its developmental phase. Even when allowance is made for a similar lag in the case of the Basket Makers, there is a definite hiatus. The Pueblos probably occupied the cave proper only during inclement weather. Most of their time appears to have been spent outside on the talus where, under the shelter of the overhanging cliff, they erected rough retaining walls of stone, levelling the areas between with a fill of stones and rubble, to form a series of terraces where they placed their storage pits, built a fireplace, and carried on domestic activities. Potsherds found there and in the upper level of the cave are characteristic of the ceramic types in vogue during the late developmental Pueblo period and demonstrate contemporaneity in the use of the shelter and the cave. No earlier material was found in the shelter. The top several inches and recent surface of the deposits in the cave had been disturbed, but they gave no indication of aboriginal occupation subsequent to that of the Pueblo II group.

On the basis of the dendrochronological time chart, the tree-ring calendar, developed for the Pueblo area and allowing for peripheral lag, it seems probable that the Pueblo horizon in the cave belongs in the 11th century. The Basket Maker occupation would

date from about 500 to 800 A.D., and the earlier horizons would antedate the Christian Era. From geologic studies made in the area at the time of the Gypsum Cave investigations the oldest human material there obtained was judged to be about 8,000 years old. The second period of occupation in Etna Cave would be approximately the same age, while the first tenants may have preceded them by several centuries. When the native horse, camel, and ground sloth became extinct is not known, but they seem to have persisted longer in this district than elsewhere. In any case it is quite likely that the two bottom levels in Etna Cave are Early Recent in age.

The materials obtained from the cave were divided between the Southwest Museum and the Boulder Dam State Park Museum at Overton, Nev., and may be seen at those institutions. Items about the work were printed in the Masterkey, bi-monthly publication of the Southwest Museum, and a detailed report prepared by Mr. S. M. Wheeler was issued in a limited mimeographed edition by the Nevada State Park Commission at Carson City in 1942.—FRANK H. H. ROBERTS, JR.

THE CASE OF TELEVISION

WHILE military expediency must still enshroud in secrecy much of the startling progress that the science of electronics has made in the last few years, we may anticipate that the effect of this progress will be rapid in postwar years. Probably the general public will be most conscious of developments in the field of television.

This progress in electronics will make possible such vast improvements upon prewar television that perplexing problems will inevitably be raised of both an economic and legal character. Prewar television was already on the market before the war and was sufficiently good to give satisfactory entertainment. The reproduction on the screen of the receiver was made up of twenty-five thousand picture elements, corresponding closely to a ten-inch half-tone engraving, made up of fifty-five dots in the linear inch. Postwar television will make possible five hundred and eighty-five thousand picture elements, corresponding to a half-tone engraving with eighty-five dots to the inch, and therefore render a vastly superior reproduction.

To realize these new possibilities in reproducing a picture of much finer texture, and in natural colors, there will be needed a sixteen megacycle band-spread in the radio spectrum of frequencies, as compared with the six megacycle band used in prewar television. The Federal Communications Commission is already studying the problem of the advisability of allowing a sixteen megacycle band to television broadcasters, for every megacycle band is jealously guarded throughout the radio frequency spectrum.

Unfortunately the problem is not merely a technical one but one of economics as well. For it is not possible to bring about these improvements in television by any adaptation of old standards. The proposed postwar improvements will render obsolete some two hundred million dollars' worth of investment in prewar television equipment. Manufacturers who were already in production of television sets, when the war interrupted, stand to lose, as do also those who have invested in television receivers made under prewar standards. If the public are to be protected against premature obsolescence of their television receivers, then the proposed improved television may be indefinitely postponed, for it appears at present that the broadcasting of television on the new standards is wholly incompatible with maintaining prewar television.

Not only will the consuming public who have already bought television receivers be involved, but it is estimated that the manufacturers and broadcasters have twenty million dollars already invested in television equipment that will become obsolete when the improved standard is adopted. On the other hand, to adopt the new standards of television immediately after the war will necessitate a considerable lag in merchandizing before new equipment can become available. This unpleasant interval may be shortened if through the cooperation of government and industry the new and better picture can be demonstrated within a year after the close of the war. In the meantime present broadcasters could continue to supply programs to those who already have prewar

television receivers. When the new system is inaugurated, old transmitters would be shut down and the prewar receivers would become immediately obsolete and unworkable.

Under the new system, however, television would undoubtedly enter a era of expansion never possible on the old system. One may well remember that the good can often be the enemy of the best, and it would appear that the financial loss and inconvenience that would be entailed in the change-over would be a relatively small price to pay for so great an improvement in the art. In the long run the American public will seek the best that science can offer. Television would not appear to present any exception. Probably those who have already invested in television equipment realized that this new art was in the pioneer stage and such adventurers will be the first to be eager to replace, even at considerable loss, obsolete equipment with new equipment of such outstanding improvement as appears possible under the proposed standards.

The question is naturally asked whether improvements cannot be made in the existing television band without sending to the junk pile millions of dollars worth of equipment. Our engineers differ as to the degree of improvement that can be effected without complete abandonment of the old standard. There seems little question that bigger and better and more realistic reproductions will be possible only on the proposed new frequency band. It is not at all unlikely that the proposed sixteen-megacycle band-spread could be curtailed to fourteen megacycles without unduly cramping the 9.5 megacycles within this band necessary to produce the picture itself. The use of side band transmission methods, already developed by General Electric and the American Telephone and Telegraph Company, may make this possible under the proposed sixteen-megacycle requirement. Color pictures with nine hundred thousand elements would then be practical, and the advantage in realism of introducing color is obvious.

The coming of television to the motion picture theater is not far off. Under prewar standards a television picture blown up to fill a twenty-foot theater screen would probably be too coarse to view at a distance of less than eighty feet. On the other hand, the proposed postwar pictures could probably be viewed at a distance as close as forty or fifty feet, representing a difference of ten rows of seats. Similar advantages would ensue in the projection of television pictures on the three- or four-foot, conventional home screen.

It is inevitable that there will be sharp contention between conflicting interests. It is to be hoped that shortsightedness and political influence may not unduly retard progress, for in the long run we must inevitably give way to the march of science in television.—HARLAN T. STETSON.

BOOK REVIEWS

STRUCTURAL GEOLOGY

Structural Geology. Marland P. Billings. Illustrated. xi + 473 pp. $4.50. 1943. Prentice-Hall.

RELATIVELY few of the divisions of the geological sciences have evolved to the point where quantitative techniques can be utilized commonly. Progress in applied and laboratory geophysics, in seismology, and in the application of mechanics in geology is building an increasingly secure foundation for the field observations and interpretations of the structural geologist. Considerable time, however, is likely to elapse before structural geologists generally will become "precision scientists." Most of them must be content to become acquainted with the concepts and language, and perhaps the spirit of precision technology, and then take off on what literally amounts to flights of controlled imagination.

The book under review unconsciously embodies this concept by beginning with a chapter on "Mechanical Principles" and including others on "Structural Petrology" and "Geophysical Methods". Such materials constitute some of the sharper tools for the analysis of surface structure, which in turn leads to the interpretation of the invisible architecture beneath.

Structural Geology is a straight-forward textbook for advanced students; it is not written nor embellished to tempt a larger patronage. It does not soften the student's approach to the various topics. For example, the discussions of folds and faults begin with chapters on nomenclature and classification and then proceed to the field criteria and examples from the field. To teachers who like to introduce subjects by observation, i.e., illustrative examples, the book under review will prove disconcerting. However, most teachers will welcome the fifty-five pages of well-organized laboratory exercises.

Professor Billings, Mrs. Billings, and many graduate students under their direction and inspiration have contributed substantially to our knowledge of the structurally and petrographically complex tectonics of New Hampshire. In their field work a variety of tools have been utilized and various unusual structural types have been recognized. To some extent this New England influence is manifest in the text. Rather than constituting a personal bias, this influence serves to give the volume unique and valuable quality. The attention paid to igneous rock structures and petrofabrics adds an emphasis which has, in the reviewer's opinion, been passed over too lightly in most general structural geology.

Structural Geology is not a layman's reader; it may well serve as a reference for terminology and principles. It will illustrate to the interested scientist the trends of thought of modern structural geologists and the extent to which application of principles and methods of allied sciences is operative in one of the earth sciences.—MAJOR ALLYN C. SWINNERTON.

THE FLOOR OF THE OCEAN

The Floor of the Ocean, New Light on Old Mysteries. Reginald Aldworth Daly. The Page-Barbour Lectures at the University of Virginia, 1941. Illustrated. x + 177 pp. $2.50. 1942. University of North Carolina Press.

THE "floor" of the ocean by its very inaccessibility has always been obscured by a certain degree of mystery even to students of earth history. From that distinguished geologist, Professor Daly, comes a characteristically forthright synthesis of much data on this subject not generally available heretofore. Professor Daly's latest book is the result of his three lectures delivered at the University of Virginia in 1941. Separated into three chapters, the first describes modern knowledge and theories of what is beneath the floor of the ocean—"the foundations of the deep" and the supposed nature of the earth. The second chapter describes what is known and conjectured about the broader phases of ocean relief, submarine mountains, coral atolls, and volcanic islands. The third chapter deals with the submerged continental shelves and continental slopes and the enigma of the deep submarine canyons and valleys indenting these shelves.

The author's picture of the foundations of

the deep is inferred from geological and geophysical evidence. Long earthquake waves that have passed under many of the oceans indicate a dense rock layer, and this he supposes extends down to a depth of about 50 miles, with possibly "broad veneering patches" of softer rocks. It is concluded that the evidence from studies of gravity, seismology, and geology all point to the existence of a thick layer of exceedingly weak rock that begins at a depth of about 50 miles. This substratum is hot and vitreous according to Daly, and its weakness is due to its yet uncrystallized form. It is assumed that a soft vitreous layer would react to earthquake waves like a solid, but slower stresses would affect it like a viscous liquid.

The results of earthquake studies have supplied a large share of what little is known about the interior of the earth. It should be stated, however, that seismologists do not generally accept Daly's picture of the earth's interior. About the only undisputed discoveries attributed to seismology are the crustal layer, thin or absent over some oceans, and a central core which responds to seismic waves like a liquid. Until many more observations are available from seismographs, especially those located on oceanic islands, a theory that will satisfy all geologic and geophysical ideas on "the foundations of the deep" is not likely to be formulated.

The ordinary reader who wonders about the origin of submarine mountains, oceanic islands, and volcanic activity must be hopelessly confused from the evidence now available. He may gain some satisfaction from the author's own change in view from some of his earlier ideas of volcanic island origin to those announced in the lectures under review. The latter seem to fit better the evidence at hand.

Volcanic islands are attributed to extra masses of rock built up on the "floor" of the ocean from the deep glassy substratum. This molten material is assumed to be world encircling, and issues through vents in the crystallized crust. Although it is reasoned that the strength of the crust is competent to support stably the volcanic island loads, the possibility of horizontal displacements of blocks of the crust of continental dimensions is considered. The regions of the East Indies and West Indies are classed as "mountain chains of Alpine complexity of structure." A convincing demonstration is given that coral atolls and fringing reefs are not upgrowing reefs based on intermittently subsiding volcanic islands; but, "according to better reasoning, the living reefs have grown up from banks and shoals during a slow late-Glacial and post-Glacial rise of sea level all over the world."

One of the greatest discoveries of the present century in the field covered by the book is the fact that the continental shelves and the continental slopes are so different in physiographic relief. The third chapter comprising almost one-third of the book covers the "continental terraces and submarine valleys." Through the wide use of echo sounding (and more reliable offshore control of position in coastal surveys) submarine valleys of distinctly fluvial characteristics have been found in the continental slopes. So striking is the evidence that these canyons have been excavated by streams to depths of more than 10,000 feet below the present sea level, that any other explanation seems impossible. At the same time, any change in level between land and sea of the order of 10,000 feet in Pleistocene time does not fit existing geologic theories. Daly's original suggestion of density currents to account for these deep valleys in the continental shelves, further developed in these lectures, is the only hypothesis which does not seriously disturb older geologic theories. Briefly the idea is that the storm waves and currents of Pleistocene times acting on the shallow continental shelves so agitated the sediments of the shelves that strong submarine currents of dense mud-laden water rushed down the steep continental slopes and eroded the existing canyons. The author cites especially the experimental evidence obtained by the Netherlands Geologist, P. H. Kuenen, to support this hypothesis. He apparently considers connections with existing land drainage as accidental, which to this reviewer is unacceptable in view of the striking evidence available in such instances as the Congo, Hudson, Mississippi, Columbia, Cagayan, Ganges, Indus, Mindanao, and other major drainage systems of the world. The author mentions the fact that the only thoroughly

sounded areas of the world continental slopes are those of the coastal areas of the United States. It should be added that the coasts of Alaska and the Philippines are in the same category. Even the deep submarine shapes of the so-called volcanic islands used so often in the literature are inferred from such meager data that one cannot consider them conclusive.

The density current hypothesis does not do violence to geologic ideas based upon the concept of relatively permanent sea level; that is, eustatic changes in sea level in Pleistocene or Recent Time not exceeding the order of 300 feet. Although the author suggests that density currents may now be operating in some regions where continental shelves are narrow, no conclusive evidence has been found that the process is in operation anywhere in the world today. Conditions favorable to such bottom currents are supposed, however, to have been effective during Glacial Periods.

The exploration of the continental shelves and slopes and oceanic areas by echo sounding has opened a broad vista into geologic history. When the present destructive interlude is over, we may hope to have a wealth of new data on this subject available to science.

Professor Daly's stimulating lectures will be enjoyed by many readers. His forthright style and lively imagination coupled with his intimate knowledge of earth sciences are well known to American geologists. In this book his capacity for applying "the principle of mutual dependence among the sciences" is thoroughly demonstrated.—PAUL A. SMITH.

HEALTH AND HYGIENE

Health and Hygiene. Lloyd Ackerman. Illustrated. xii + 896 pp. $5.00. 1943. Jacques Cattell.

THE author has produced a well-documented work free from dogma and of value to the student, the lay reader, or the teacher. Each chapter is followed by a truly imposing array of references.

The first chapters are largely historical. They could be improved by a relation of the history, structure, and functions of public health services, both national and local, and a history of medical education in America. Some of the space given to descriptions of the cults could have been so used to better advantage. Otherwise the book is praiseworthy. The literary style is excellent and entertaining, the print is clear, and the references seem to cover the field entirely. Emphasis is laid on mental hygiene, and the treatment is subjective.

Mental diseases are covered as fully as necessary in a work of this sort, and the terms used are reduced to the understanding of the reader who may not have a scientific training. This does not detract from its scientific value; rather it should enhance its value for the student just being introduced to the subject.

The text on vaccines, serums, and vitamins has been stripped down to understandable terms. However, this is so generally true of the whole book that it is superfluous to take up chapters separately.

All in all we have here a scientific treatise on health and hygiene that can be recommended highly.—M. F. OSBORN.

A PRIMER OF ELECTRONICS

A Primer of Electronics. Don P. Caverly. Illustrated. xi + 235 pp. $2.00. 1943. McGraw-Hill.

REALIZATION that knowledge of the general physical principles on which modern electric devices are based had not been made available to even a small fraction of the number of interested people prompted the preparation and publication of this book. Through the book the nontechnical reader can obtain a brief survey of the broad field of electricity, magnetism, and related topics of physical science. Discussion of basic principles is cleverly interspersed with descriptions of their practical application to form an interesting and enlightening review of a broad field.

Electronics is defined in the early part of the book as "that branch of science and technology which relates to the conduction of electricity through gases or in vacuo." The book, however, includes discussions of ultraviolet and infrared, as well as visible, portions of the spectrum. These topics, which are not clearly included by the terms of the definition of electronics, make an interesting addition to the text. At the same time, the book would have been more satisfying, at least to the reviewer, if such discussions had

been omitted or curtailed and an equivalent space used for further explanation of the field of electronics in a more restricted sense. In particular, if the sections on capacitive and inductive reactance could have been expanded to provide a background for an adequate explanation of resonance in radio frequency circuits, the average reader would probably have had his interest stimulated further.

The fundamental principles of vacuum tubes and their simple application in radio circuits are clearly described with the aid of excellent illustrations. Considering the complexities involved, the discussions of television, photograph transmission, frequency modulation, the electron microscope, and other relatively new vacuum tube applications are well done. The average reader, whose background probably includes elementary high school science, will find much of interest and value in this book. It is recommended for such people if they have any feeling of curiosity as to the how and why of the wonders of modern electronics.—GAIL F. MOULTON.

MEDICAL PHYSICS

Medical Physics. Otto Glasser, Editor-in-Chief. xlvi + 1744 pp., 1382 illus. 1944. $18.00. The Year Book Publishers, Inc.

MEDICAL physics is not exactly a new subject. Its beginning extends to the Middle Ages, but the tempo of applications and discoveries has become rapid in the last ten to twenty years. There has grown up a truly stupendous amount of scientific information in this borderline field between physics and medicine. It has, however, suffered from being widely scattered in publications some of which frequently are inaccessible to those most interested.

The text which is the subject of our review is really an encyclopedia in form and scope. The editor and his twenty-two associate editors are to be congratulated that the "small manual" idea of treatment was discarded at this stage of the subject in favor of a more complete and comprehensive treatment. They are further to be congratulated upon enlisting a corps of 245 different specialists, each of whom has prepared articles on one or more subjects. Their 255 articles which comprise the text of this book are arranged alphabetically under the titles. A list of twenty-three subject sections with the number of articles (*Ar.*) in each, the number of different authors (*Au.*), and the name of the associate editor follows:

Subject	*Ar.*	*Au.*	*Associate editor*
Anatomy	23	22	Normand L. Hoerr
Bacteriology	5	6	Otto Rahn
Biometrics	4	4	Charles P. Winsor
Biophysics	41	48	Otto Glasser
Dermatology	4	4	George W. Binkley
Hematology	18	12	Eric Ponder
Medicine	104	113	Russell L. Haden
Neurology	20	25	W. James Gardner
Nuclear physics	11	11	Robley D. Evans
Ophthalmology	7	8	A. D. Ruedemann
Optics	15	15	W. B. Rayton
Orthopedics	12	12	James A. Dickson
Otolaryngology	9	8	Paul M. Moore
Pathology	31	36	Harry Goldblatt
Pediatrics	1	1	Norman C. Wetzel
Photography	16	16	Leo C. Massopust
Physical chemistry	27	28	F. M. Whitacre
Physical therapy	29	35	Walter J. Zeiter
Physics (methods)	58	66	John G. Albright
Physiology	101	111	Harold D. Green
Radiology	67	72	Harry Hauser
Surgery	40	48	Frederick R. Mautz
Urology	15	17	Charles C. Higgins

The text is printed in double columns, and the excellent illustrations are numbered separately for each article. The working formulae and tabular material are noteworthy features of the book. A suggested bibliography in brief form is appended to virtually all of the articles.

The extensive subject index, which comprises 44 pages of three columns to the page, is intended to unify this book. The analysis is detailed and appears to be entirely adequate. The subject index is followed by a name index of 16 pages, four columns to the page.

The material covered ranges widely and embraces the whole meeting ground of physics, medicine, chemistry, biology, engineering, and other sciences. Under nuclear physics, for example, occur such articles as descriptions of betatrons, cyclotrons, Geiger counters, and a discussion of tracer techniques. Under medicine occur such topics as specific gravity and solids of blood, effect of heat and cold, exercise and posture, motor functions, and fever therapy. Then there are splendid discussions of such subjects as chlorophyll,

bio-electricity and cinematography. Some of the subjects seem to have been covered in the fullest way, as for example the exhaustive treatise on photometry, covering 33 pages with 5½ columns of bibliography. No effort seems to have been spared to get the best of authorities in the particular fields. Zworykin, for example, writes on the electron microscope, Wollan on cosmic rays, Trump on supervoltage generators, Rashevsky on mathematical biophysics, to mention only a few names immediately familiar to physicists.

It would require an entire faculty of a university in which such schools or departments as physics, chemistry, mathematics, biology, physiology, and medicine were particularly strong to give a critical evaluation of the discussions. It appears to the reviewers to constitute a necessary text and reference work for all specialists either within or on the edge of this field. It will be useful for consultation by students and teachers particularly as a reference work for concise, up-to-date discussions in this field. The reviewers are impressed with its apparent usefulness also to practicing physicians and experimental medical scientists.

The editor speaks of the book as being designed to serve as a *textbook* and as a *working instrument*. Actually it is doubtful if it will serve very well as a textbook, which in this field is long overdue. What is needed, however, is a book of about one-fifth the size of this one, and leaning heavily on it for information and inspiration. This book will no doubt hasten the advent of such a text now that the material is accessible. The encyclopedic arrangement is indicative of the main purpose of the book; namely, to serve as a basic reference work in libraries and hospitals and for every physician and scientist who works in the borderland between the pure sciences and medicine.

The publishers are to be commended for undertaking the expensive task of publishing so comprehensive a work, but they have succeeded well. The reviewers find it remarkably free from typographical errors.

It may be expected that this work will result in increased emphasis on this subject in schools of both physical science and medicine. It is likely to stimulate research in this field. It is difficult to see how the conscientious researcher can afford to be without the information here assembled in ready form.—ROGER C. SMITH (Biology) and M. H. TRYTTEN (Physics).

PRINCIPLES OF BEHAVIOR

Principles of Behavior. Clark L. Hull. x + 422 pp. $4.00. 1943. Appleton-Century.

THIS book vigorously denies that any branch of the social sciences is inaccessible to objective mathematical inquiry. The first two chapters are devoted to a luminous exposition of the nature of scientific and objective behavior theory. In these lucid pages Dr. Hull declares that ". . . natural science theory is aggressive towards the problems of nature, and it uses logic as a tool primarily for mediating to the scientist himself a more perfect understanding of natural processes" (p. 8). This is a far cry from the baby-with-bathwater objectivism of the Watsonian era. Science ". . . has postulates but no axioms in the Euclidian sense . . . scientific theory reaches belief in its postulates to a considerable extent through direct or observational evidence of the soundness of its theorems" (p. 9). Hull rejects "emergentism" as a pseudo-remedy and a doctrine of despair, maintaining that supposedly impossible derivations with regard to goal or purposive behavior are actually possible by reference to stimuli and movement.

The intricacy of such concepts as intelligence, insight, goals, intents, strivings, and value is fully realized by the author, who nevertheless states ". . . the relative backwardness of the behavior sciences is due not so much to their inherent complexity as to the difficulty of maintaining a consistent and rigorous objectivism" (p. 28).

After this prologue Hull proceeds to derive sixteen basic molar postulates and five major corollaries from a detailed examination of numerous experimental studies in the field of animal behavior. The complicated equations which support these postulates are relegated to terminal notes after each chapter. A comprehensive glossary of terms is also included.

The author offers a theory of primary motivation which states that ". . . the potentiality of response evocation is the product of a function of drive intensity multiplied by a

function of habit strength." Drive itself is a logical construct ". . . since it cannot be observed directly any more than can effective habit strength" (p. 390). Drive stimuli become integrally associated with the habit involved through conditioning to the reaction which is associated with reduction of a need.

The concept of "behavioral oscillation" is introduced to account for the failure of effective reaction potential to function fully in the evocation of action. Oscillation, or the variability of response from moment to moment in the presence of a constant stimulus situation seems to distribute itself in the form of a Gaussian curve. This little-understood physiological process ". . . is in a large measure responsible for the fact that the social sciences must pool many observations before ordinary empirical laws become manifest. Thus natural laws in the social sciences must always be based on statistical indices of one kind or another" (p. 393). Is this not also true of the physical sciences? Behavioral oscillation ". . . has doubtless appreciably retarded the development of the behavior sciences" (p. 393). Apparently complexity, after all, must share honors with lack of objectivity in retarding the growth of these sciences.

Hull's prophetic credo hurls a ringing defiance against the decadent doctrines of Emergentism, Intuitionism, and similar defeatist theories. "Progress in this new era will consist in the laborious writing, one by one, of hundreds of equations; in the experimental determination, one by one, of hundreds of empirical constants contained in the equations . . . in the ruthless discard or revision of once promising principles or concepts which have failed wholly or in part to meet the test of empirical validation."

This book is more than a technical discussion and derivation of basic principles. It represents a bold and imaginative affirmation of faith in scientific method when the flame of that faith is flickering in the winds of the world holocaust. For intellectual inspiration and inspiration in seminars on scientific method and psychology it will have few equals.—FRED BROWN.

AMERICAN PSYCHIATRY

One Hundred Years of American Psychiatry. Published for the American Psychiatric Association. Illustrated. xxiv + 649 pp. $6.00. 1944. Columbia University Press.

THIS handsomely produced volume marks the centennial of the American Psychiatric Association, the oldest national medical organization in America. Thirteen contributors, between them, deal with such subjects as the development of psychiatry from colonial days to the foundation of the Association, psychiatry in Europe at the middle of the 19th century, the founding and founders of the Association, the history of American mental hospitals, psychiatric research in America, American psychiatric literature, the history of psychiatric therapies, the history of mental hygiene, military psychiatry in the Civil War and first and second World Wars, psychology in relation to psychiatry, American psychiatry as a specialty, legal aspects of psychiatry, and psychiatry and anthropology. These papers are most attractively written, and contain a remarkable amount of useful information interestingly presented. The book is well illustrated and contains an excellent index.

American psychiatry may well be proud of its hundred years of achievement, and in keeping with the high standards of that achievement the present volume forms a fitting centennial celebration of that event. The reviewer enjoyed the privilege of attending the centenary meetings of the American Psychiatric Association held in Philadelphia in May, 1944, and he can testify not only to the success of that meeting but also to the glowing health of psychiatry as a branch of medicine.—M. F. ASHLEY MONTAGU.

COMMENTS AND CRITICISMS

Fair and Warmer

Weather forecasting may be a science in the United States, but it's still an art, romantic and elusive in Colombia. Doubtless the air lines even there manage things differently, but the daily paragraphs headed "Meteorological Data" that appear in *El Tiempo*, the Times of Bogotá, Colombia, are literary gems, polished, cultured, urbane—and vague.

"Increasing cloudiness and slightly warmer today and tonight. Tomorrow partly cloudy and warmer, with a thundershower in the afternoon." Never has such a crass, utilitarian forecast appeared in the leisurely pages of *El Tiempo*. Where would be the poetry, the erudite reference, the literary style? Factualism is for the gringos, say the Latin-Americans; we'll take literature.

"It has been reasonably said," writes Don Guillermo Bonitto in one of his paragraphs of meteorological data in *El Tiempo*, "that new things may be found in old books. Indeed, since the colonial epoch (according to incomplete records), the climate of Bogotá has been characterized by high indices of humidity during cycles or periods fluctuating between five and six months in length; in such periods even the months of July and August have been rainy and very cold as at present, a condition that will very probably continue until the end of the month."

Don Guillermo, the writer of the daily paragraphs, is obviously a gentleman who takes the larger view. Not for him the mere twenty-four hour forecast, the precise anticipation of maximum and minimum temperatures, the bare details of wind direction and precipitation. Not for him the paraphernalia of isobars and humidity symbols, barbed arrows and austausch coefficients. Weather forecasting for Don Guillermo is a branch of literature.

Once he began his daily paragraph with unaccustomed precision: "Probable condition of the atmosphere: strong sunshine followed by rain." He quickly repented, however. The next sentence indicated his difficulty: "The laws of physics and mathematics," he wrote, "so wisely combined by the Supreme Divinity, stand in contradiction to the limited knowledge which human science has been able to acquire concerning the relationship existing between them." A becoming humility, no doubt, but not very helpful to the picnicker.

Don Guillermo's work as a weather forecaster evidently keeps him constantly aware of the contrast between human science and divine wisdom. The intermingling of human and divine, he points out on another occasion, is reflected in the combination of science and religion that erected the temples of the Assyrians and Chaldeans as well as those of the Muiscas and Sugamuxis right in Colombia. Full of mystery as those solid, tangible structures are, who are we moderns that we pretend to understand, much less foretell, the vagaries of the elusive, intangible atmosphere?

But Don Guillermo is at his best when he is most historical. "These epochs of abundant rain," he writes (it rains almost all the time in Bogotá), "agree with the records left us by some of the Conquistadors, records later studied by Humboldt, the marvelous Caldas, Toaldo, and other authors who labored in the field of meteorology, a science very useful for progress, but still in its cradle here in the tropics."

With considerable justice Don Guillermo is consistently pessimistic about the weather in Bogotá. Rank amateur though I am, I can testify that the temperature hovers around fifty degrees Fahrenheit the year round and that it rains continually, now drizzling, now pouring. Old residents sadly assert that six totally clear days in any one year would establish a record. Thus, after one spell of clear weather, Don Guillermo reminded his readers of the inevitable. "According to careful studies," he wrote, "the three days of splendid sunshine now enjoyed in Bogotá will be succeeded by our customary weather within another day."

Sometimes Don Guillermo takes time out to reply to his correspondents. "Some kind readers of these essays," he writes, "have asked the author to write at greater length. Gladly would he comply if he were a writer; but, aside from his complete lack of such talent, he adheres to the doctrine of expressing ideas in the most concise, brief, and concrete form possible, even though he make mistakes, and he does with considerable frequency."—JAMES PAUL STOAKES, Marshall College, Huntington, W. Va.

Prometheus Bound

In the March, 1944, issue of THE SCIENTIFIC MONTHLY, which has just reached this distant spot in New Guinea, there is an article by Dr. Pei Sung Tang entitled "Helios and Prometheus: A Philosophy of Agriculture." Three statements therein are believed to be so unscientific in character that they should be discussed further, lest readers unfamiliar with the progress of science in certain fields give undue weight to the fact that the assertions appear in THE SCIENTIFIC MONTHLY.

The first of the three is found in the fourth paragraph, where the author says, "He [man] is not only constantly subjected to the Malthusian struggle between himself and his fellow men; . . ." This apparent indorsement of the so-called "Malthusian Theory" will be surprising to every scientist who has troubled to read Malthus' exposition of this notion,

because a less scientific combination of geometrical and arithmetical progressions, unsupported by anything that a scientist would consider relevant data, would be difficult to imagine. The widely used but little read "Essay on Population" would hardly deceive a college sophomore today, and THE SCIENTIFIC MONTHLY would, I feel certain, immediately reject a manuscript so obviously fallacious; unless, indeed, you chose to publish it as an illustration of the absurd results sometimes obtained by failure to apply the scientific method.

It is hardly possible that those of your readers who are active in applying the scientific method will not immediately see the absurdities involved in Malthus' "theory." For those who would prefer to read a scientific analysis of the "theory," I suggest reference to the works of a writer whom John Dewey has called one of the few great thinkers who have left a written record of their work since the dawn of recorded history. In Book II of "Progress and Poverty," Henry George subjected the "Malthusian Theory" to a scientific analysis that, were it not for the perennial rebirth of great delusions, would have long since disposed of the "theory."

The second point deserving further consideration is Dr. Tang's assertion, "Until the time when he [man] can reproduce this apparently simple process of photosynthesis in his laboratory, independently of the green plant, man cannot claim to be free from the vestiges of his ancestral sun and fire worship." It hardly seems possible that Dr. Tang is here using words in a scientific manner, that is, for symbolic rather than poetical purposes. Possibly a poor translation has occurred. In any event, there is no apparent reason why primitive superstitions should continue, even in part, until man has solved any particular scientific problem. Probably most scientists would claim to be free of any vestiges of "ancestral sun and fire worship," and I think their claims would be justified.

The third point deserving further consideration is found in Dr. Tang's statement, "So long as the population of the world does not decrease, so long as the present practice of agriculture cannot be expected to give significant increase in crop yield, the competition between man, animal, and plant for land area will forever be a barrier to providing animal products for human consumption." The implication of this statement seems to be that the *future* practice of agriculture cannot be expected to give a marked increase in crop yield. (Obviously, the "present practice" can be nothing but the "present practice" in any event; only the future practice has any chance of increasing the crop yield. I am assuming that Dr. Tang intended to say something of significance.)

Now, one of the more interesting developments of recent decades has been the very great increases in crop yields that have resulted from crop rotation, more effective fertilization, and particularly from the use of more carefully selected seed resulting from scientific cross-breeding of various plants. Not being an expert in the latter field, I may have chosen the wrong technical phrase to describe the procedure, but most readers will no doubt understand my reference. In view of these developments, and in view of the primitive and inefficient farming procedures still followed by most of the farming population of the world, I believe that the future practices of agriculture can be expected to give significant increases in crop yields.

It may well be that improved practices will not be readily adopted in such countries as India and China, but this will not be because improved practices "cannot be expected to give significant increase in crop yield." It will be for the simple reason that any significant increase in crop yield would be of no benefit whatsoever to the tax burdened, rent racked, and bandit ridden "share croppers" of India and China. They are now regularly stripped of all but that needed to keep life in their bodies, and, as they know all too well to their sorrow, a more bounteous harvest but increases the greed of their extortioners. All incentive for adopting improved practices has been taken from them, to such an extent that when crops are even slightly subnormal thousands starve in order that the tax gatherers, and other "bandits" may continue to live in the style to which they have become accustomed.

There is no doubt that modern methods and utilization of land not now producing food (including such vast areas as the valley of the Amazon, now being "pioneered" as our middle west was a century ago, and those large and tremendously fertile areas of which New Guinea is an example) could result in multiplying several times over the present crop and animal products of agriculture. It is not to the niggardliness of nature nor to the backwardness of scientific progress in agriculture that we should look for explanation of the undernourishment described by Dr. Tang.—COLONEL E. C. HARWOOD, C.E.

Hybrid

Reference is made to a paper entitled "The Forbidden Land" in the May, 1944, issue of THE SCIENTIFIC MONTHLY. In Figure 10 on page 355 there is a picture of an animal described as a yak. This is apparently not a yak, but a hybrid between domestic cattle and the yak, which the Chinese refer to as a *pian niu*.—RALPH W. PHILLIPS.

Better Late . . .

The photograph of James McKeen Cattell appearing in THE SCIENTIFIC MONTHLY of April, 1944, is derived from the *Journal of Consulting Psychology* (copyright 1937). Ex post facto acknowledgment is hereby made.—EDS.